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Laws and Natural History in Biology*

WIM J. VAN DER STEEN AND HARMKE KAMMINGA

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1 INTRODUCTION

Philosophers and scientists alike have often defended the thesis that theory integration is an important ideal for science. In the early days of logical positivism the ideal took the form of a substantively unified science, with theory reduction as the proper form of unification. The thesis that science can be unified in this way soon came to be regarded as over-ambitious. It was succeeded by the weaker thesis that *methodological* unity of the sciences is desirable, and that it can be realized. Thus it was thought that good scientific theories should consist of an interconnected set of universal laws of nature, that good explanations should conform to the covering-law model, etc. Even this weaker characterization of science did not remain unchallenged. Nowadays, there is much disagreement about the nature of theories, laws and explanations. This also affects the evaluation of correspondences and differences between the sciences.

In this paper, we want to consider the thesis that biology has no laws or theories of its own, a thesis which keeps cropping up in the philosophy of biology ever since it was provocatively defended by Smart [1963].

To assess this thesis, one needs to have some understanding of the notions of law and theory. We assume that the following characterization of laws of nature, which is a variant of the so-called *regularity view*, is useful for the practising scientist. A statement is a law if it satisfies the following criteria: (i) it

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is general in the sense that it contains a universal quantifier, (ii) it is general in the sense that it does not mention particular individuals, times or places, (iii) it has empirical content, (iv) it is well-confirmed, and (v) it is well-entrenched (*i.e.* it belongs to a theory).

In our discussion of laws the main emphasis will be on (i), (ii) and (v), the other criteria being taken for granted. For ease of exposition we will informally use the following terms in connection with the criteria. *Generality* will indicate (i) and (ii); *natural history* points to bits of science which have no generality and so do not contain laws. *Coherence* will stand for (v), and candidate-laws falling short of (v) will be said to violate the principle of coherence. *Theories* will be viewed as sets of interconnected laws.

We would not use these distinctions in a very rigid way. Thus, in many cases scientists will rightly be content with 'laws' which are at best approximately true. Also, the distinction between laws and natural history is not a very sharp one. If there are differences in this respect between physics and biology (as we think there are), they will be a matter of degree. Indeed there are those who would argue that physics, until now, has not produced much in the way of laws in a *very* strict sense (*e.g.* Barrow, 1988). Time and again, new laws of physics have turned out to be locally rather than universally valid. Perhaps this calls for the articulation of a more restricted paradigm of generality, but we will take this for granted. Our emphasis will be on more marked deviations from generality which are characteristic of biology.

Many philosophers would argue that the regularity view is basically inadequate because (i)–(v) only represent necessary conditions which are not jointly sufficient. These conditions wrongly give *accidental generalizations* the status of law. Laws somehow represent stronger connections, they must be able to support counterfactual statements and/or they must represent causal relationships. For example, it has been argued recently (Dretske [1977], Tooley [1977, 1987], Armstrong [1983]) that there must be a modal connection of necessity between properties in a universal statement if it is to count as a law (for critical comments, see Niiniluoto [1978], Hochberg [1981], Forge [1986], Urbach [1988]). The idea is that laws entail ordinary universal statements which satisfy criteria such as (i)–(v) above, but that the reverse is not true.

It seems to us that the criteria involved in the regularity view need not be as weak as opponents suggest. Specifically, we think that a proper explication of coherence may well result in the exclusion of accidental generalizations. Even if the critics are right, the theses we will defend need not be affected because they are meant to be robust. For example, if it is shown that biology contains few laws on the regularity view, the same will be true of laws in a stronger sense as envisaged by critics of this view.

There is yet another controversy about laws and theories which is more complex. The regularity theory was embedded originally in the so-called syntactic or received view of theories. This view sees scientific theories syntactically, as uninterpreted calculi which are linked to the level of observation by correspondence rules. Laws as general *empirical* statements, therefore, are arrived at only after a process of interpretation. The so-called semantic view, by contrast, reconstructs theories as ideal systems with a semantic but no empirical interpretation (for a recent defence of this view in the context of biology, see Thompson [1989]). Empirical matters are covered by statements which identify some empirical system with an ideal system. We would regard statements of the latter kind as the equivalent of laws under the syntactic view (if they satisfy criteria such as generality). However, semanticists often use the term 'law' for different entities, viz. statements describing the behaviour of ideal systems. As far as we are concerned, this is a terminological rather than a substantive issue (for further comments, see Sloep and van der Steen [1987] and the discussion following the article).

Much of the literature approaches the issue of laws in biology with general philosophical arguments. We think that empirical matters should get more attention. Biological research will have to show what laws (in any reasonable sense of the term) can be elaborated. To underline this, we will proceed as follows. First, we will consider a sample of common, relatively abstract philosophical arguments which we regard as at best inconclusive. Next, we will analyse arguments which are closer to biology, but which are biased because they cover only a limited area of biology (systematics and evolutionary biology, the paradigms of current philosophy of biology). Then we will proceed with case studies from other areas of biology which together suggest that laws and natural history, and anything in between, deserve a legitimate place in biology.

2 INTRODUCING THE PROBLEM: SMART AGAINST LAWS IN BIOLOGY

According to Smart [1963], only physics and chemistry have 'laws in the strict sense', *i.e.* laws which are universal 'in that it is supposed that they apply everywhere in space and time' (*cf.* our criteria (i) and (iv)), 'and they can be expressed in perfectly general terms without making use of proper names or of tacit reference to proper names' (p. 53) (criterion (ii)). As an example, Smart discusses the proposition that albinotic mice always breed true. He argues that mice are defined by their place in the evolutionary tree. Therefore, the term mouse carries implicit reference to our particular planet, earth, and this makes statements about mice violate (ii). Of course one could define 'mouse' in a different way, by reference to properties possessed by mice, but if that is done, there is no reason to suppose that statements about mice are true throughout the universe (*cf.* (iv)). For all we know, some planets might have mice which do not breed true although they are albinotic.

Why is it that physics and chemistry are in a better position? Biologists cannot investigate mice which may live somewhere in Andromeda, but physicists have similar handicaps. The point, according to Smart, is that the laws of physics inspire more confidence since physics works with relatively simple or homogeneous systems with constituents such as elementary particles which are believed to be ubiquitous in the universe. Not so for the complicated and idiosyncratic structures of biology. Here we are dealing with applied physics and chemistry expanded with natural history.

Smart's example would not convince biologists since they would not regard statements like the one about albinotic mice as candidate-laws. Even if they would, Smart's argument would not make them feel uncomfortable. Implicit reference to the earth may limit generality, but biologists would be quite content with statements which are so general that they hold true all over our planet.

However, there are good points in Smart's analysis if it is brought to bear on more realistic biology. Firstly, implicit reference to the earth in statements with species names (*e.g.* mice) may not strike biologists as very worrying, but there is a more substantive reason for regarding them as non-general which is closer to the affairs of biology. Statements about species cannot qualify as laws of nature if species are *individuals*, and many philosophers of biology (Hull [1978]) and biologists (Ghiselin [1987], Mayr [1987]) now think that taxa *are* individuals from a logical point of view.

Secondly, Smart rightly mentions non-homogeneity of biological systems as a possible stumbling-block in the search for generality. However, he connects it with a theme which would not appeal to biologists (*i.e.* testability of statements about mice on Andromeda). We believe that non-homogeneity is interesting first and foremost because it is connected with complexity and uniqueness, features which tend to hamper the elaboration of more limited general statements concerning terrestrial life which do interest biologists.

Smart also has examples which involve real biology. He argues that even Mendel's laws cannot qualify as laws in the strict sense in view of exceptions. They simply are not generally true. He is no doubt right, but it does not follow from this that biology is different from physics. The point is whether we could elaborate laws in such a way that exceptions are accommodated. Ruse [1973], for example, argues that exceptions in the case of Mendel's laws are harmless because they can be explained by biologists. Such explanations would typically refer to lower levels of organization and include appropriate boundary conditions.

This argument shows that coherence will play a role in discussions of generality. Specifically, a particular form of coherence which unites levels of organization, reduction, is often appealed to in evaluating the generality of laws and theories of biology. We will evaluate this issue critically in the next section. Latter sections will deal with the 'species problem' mentioned above, and with the general theme of uniqueness and complexity of biological systems.

3 COHERENCE AND REDUCTION IN BIOLOGY

Smart's view of biology is extreme and it lacks a solid foundation, but it should not be dismissed lightly. With respect to laws of nature biology and physics are clearly different. As a matter of fact, biology, unlike physics, does not have many general statements which one would call laws.

It could be objected that we are overlooking the obvious since biology has *many* laws which are commonly taken for granted. Examples are statements to the effect that all organisms contain DNA, that no organism has eternal life, that circadian rhythms occur in every organism, and so forth. However, the assumption that these are general, well-confirmed statements, would not suffice to show that they are laws. One would need to show in addition that they satisfy the principle of coherence, *i.e.* that they belong to a network of interrelated general statements (a theory). There is no reason to assume a priori that this principle is easily satisfied.

Conversely, it would be unwise to reject a statement as a candidate-law simply because exceptions have been found. Ruse's [1973] comments on Smart's views are a case in point. Smart argued that even Mendel's laws, putatively a paradigm of laws in biology, are problematic since there are exceptions. Ruse replies that they are genuine laws because exceptions were accounted for in subsequent research. What happens at the level of phenotypic properties is explained by phenomena at the cellular and the molecular level. For example, some exceptions are explained by gene linkage. Genes which are located on the same chromosome do not segregate independently and so violate Mendel's first law, the law of independent assortment.

Ruse's line of reasoning is on the right track, but we have reservations concerning the conclusion that Mendel's laws are genuine laws. In classical genetics, the increase of coherence accounting for exceptions to Mendel's laws took the specific form of *vertical integration*, integration of various levels of organization. In his reply to Smart, Ruse apparently assumed that integration has taken a form which justifies the conclusion that we are dealing with laws. However, one would need a rather strong form of integration to infer this, whereas the connections between levels of organization elaborated in genetics are rather loose. The strongest variant of vertical integration, which involves reduction, is downright suspect here (see *e.g.* Hull [1981]).

In population genetics there is also much emphasis on vertical integration, although the situation there is slightly different. There are many putatively general laws in this field, but it is extremely difficult to test them. As Lewontin [1974] has argued, for population genetics to become a mature discipline, one would like to have an integrative theory which connects the level of the genotype with that of the phenotype. However, a full-fledged integration has not been forthcoming. More recently, Lewontin [1985] has suggested that the answer may come from molecular biology. Indeed, understanding of the control of gene expression in eukaryotic cells is growing very rapidly, but it is too early to say precisely how events at the molecular and cellular level interconnect with the level dealt with by population genetics. Perhaps an increase in coherence will yield general laws that are well-confirmed. As yet it is an open question.

One should not assume that vertical integration is *necessarily* involved in the construction of general theories. There is no reason to suppose that there cannot be self-contained general theories which deal with one level of organization only. Consider the following example. It is often thought that two species which need the same resources such as food cannot coexist in the same area. Competition would either lead to the exclusion of one species or to changes in resource use in one or both species. Here we have an interesting candidate-law of nature. Suppose we find an exception. It is conceivable that the explanation is that species which coexist in spite of using common resources share a predator which always feeds on the most common species. This example has almost the same features as the one from classical genetics, but it involves *horizontal rather than vertical integration*. The emphasis stays on one level of organization, that which deals with interactions between individual organisms (for analogous arguments concerning evolutionary biology, see Sober [1980] and Splitter [1988]).

Various authors have apparently failed to notice that coherence without vertical integration is possible. Moreover, the criterion of coherence is often given an unduly strong form by coupling it with reduction (mostly, but not necessarily, in the form of vertical integration). Several examples are presented below.

Hull [1974] has suggested that reduction is the only possibility to establish laws in various areas of biology. One of his examples is ontogenesis. He argues that individual steps in biochemical sequences that characterize development may well be covered by laws. But what about the sequence as a whole?

> What is needed before we can count statements of such developmental sequences as laws is the recognition of some system of laws from which they can be derived. To do this, we must enlarge our area of interest to include the genotype of the organism and how it functions to control this particular pathway (Hull [1974], p. 75).

The same line of reasoning was earlier followed by Beckner ([1959], pp. 91–2). Hull gives similar arguments in the context of phylogeny (pp. 79–80). We think that his line of reasoning represents a *non sequitur*. If *one* general statement concerning development is to count as a law, it will have to be connected with other general statements, but it is conceivable that *all* the interconnected statements we end up with are about development.

Rosenberg [1985] has applied similar arguments to a more extensive area of biology. He thinks that generality in the form of laws may be possible in biology, but only for very high and very low levels of organization (see Section 4). The following passage contains one of his arguments to the effect that general statements for intermediate levels are not feasible.

> But there is another, more fundamental source for the disjunctiveness and specificity of our knowledge of the biosphere, one that is unavoidable, given that life is the product of natural selection operating repeatedly on successive levels of organization. . . . At each level of organization there may be different arrangements, all equally fit. . . . At any given level of organization, what survives will be a function of natural selection operating on a disjunction of arrangements of equally adapted subsystems at the immediate lower level or at levels below this one. . . . The result will be that when we come to uncover the underlying mechanism of a type of biological system identified, say, by its function at one level of organization, we shall discover a variety of slightly different subsystems operating within otherwise indistinguishable systems. Thus a general description of the mechanism of operation for all examples of the type of system under examination will have to be disjunctive if it is to be complete. If there are too many disjuncts for manageability, we may never attain completeness. . . . But an incomplete description . . . cannot be a law (pp. 222–3).

Rosenberg is obviously considering research which follows a reductive strategy. He suggests that problems with reduction preclude the attainment of generality (in the form of laws) in many areas of biology. But this is a *non sequitur* since he fails to consider the possibility that there may be generally valid biological statements which stay on one level of organization. For the rest, Rosenberg's line of reasoning is problematic even if one assumes, for the sake of argument, that a reductive strategy is needed to arrive at laws. Rosenberg fails to demonstrate that a reductive strategy is at odds with biological laws. *Of course* organisms which are similar at some level of organization may have different subsystems. But there will be similarities in subsystems as well. Now one need not consider *all* the properties of subsystems in explaining some higher level phenomenon. Complete descriptions are never at issue in science. Perhaps *some* higher level phenomena under *some* description can be explained in terms of *some* identical features of subsystems. For example, the motility of different organisms and of different systems within organisms (*e.g.* striated muscle, smooth muscle, cilia, spermatozoa) at the very least requires the presence of a variety of fibrous proteins. The contractility of the structures is the outcome of the specific ways in which these proteins are organized in filaments. The fact that there is great variety of systems and subsystems does not prevent us from generalizing the connection between motility at different levels of organization and fibrous proteins.

In a quite different context, that of the historical sciences, Fales [1980] has defended an even stronger thesis with arguments which resemble Rosenberg's. He suggests that laws for higher levels of organization are problematic, and that one should aim at replacing them by lower level laws by a reductive move. In history, such a move is impossible according to him since historical processes are complex and unique. He then considers the situation in physics:

> [In physics there is] the general fact that law-governed interactions can combine in indefinitely complex ways. This fact . . . characteristically imposes upon the formulation of . . . laws the necessity of adding an open-ended *ceteris paribus* clause, or defeasibility condition (p. 262).

As a result, events to be explained cannot be deduced in a straightforward way from the relevant laws and initial conditions. But, argues Fales, in the hard sciences a strategy is available to elude this problem:

> That strategy . . . is to cash in defeasible . . . laws for the deeper explanatory laws of some reducing theory (p. 262).

The main part of Fales' paper is concerned with showing that reduction is impossible in the historical sciences. We will not quarrel with *this* part of his argumentation. Our objection is that he does not argue the premiss that reduction is the only possible strategy to deal with open-ended *ceteris paribus* clauses. Coherence need not necessarily take the form of vertical integration, and in the more specific form of reduction it may well be less prominent than Fales thinks even in physics.

Hull, Rosenberg and Fales argue in a general, abstract way. If our analysis is on the right track, their views are inconclusive since they take as their starting point the unjustified philosophical assumption that generality needs to be coupled to coherence in the form of reduction and/or vertical integration. The assumption may be correct, but it is not clear that potential counter-examples have been looked for systematically. The issues at stake are *empirical*, not philosophical ones. This is what Schaffner [1980, 1986] has emphasized. His case studies show that biological research concerned with intermediate levels of organization (the 'middle range') is characterized by vertical integration (is interlevel). They also suggest that generality is not a common feature of biological 'theories'. There is much variability within and between species, and this alone makes attempts to develop strictly universal, formalized theories futile. The examples Schaffner [1980] elaborates, the operon theory, the clonal selection theory, and the two-component theory of the immune response, clearly confirm this picture.

However, Schaffner, like Rosenberg, thinks that laws *are* possible in biology for high and for low levels of organization. His arguments to this effect, as they stand, are less compelling. Universality of the genetic code is one of the examples, which he seems to take for granted. (In fact, more and more exceptions are being found.) Here his emphasis is on generality rather than coherence. But would 'theories' of the genetic code be theories in a strict sense of the term which also satisfy the principle of coherence? In order to show that one would need to reconstruct the relevant area of biology to uncover a *network* of general statements. We tentatively offer some pointers towards such a reconstruction. First, one might take into account von Neumann's formal requirements for a self-producing system (von Neumann [1951]), which establishes the need for a code by which genetic instructions are translated so that they can be read by an 'assembly machine' (the ribosome). Secondly, arguments have been presented in the literature which suggest that the co-existence of more than one genetic code would inevitably lead to an unstable situation and lead to the elimination of all but one (Chernavskii and Chernavskaya [1975]). The universality of the genetic code for all terrestrial organisms would thus be given a theoretical basis (although slight deviations in the code of mitochondrial and chloroplast DNA, among others, require further explanation!). The precise nature of this code, however, would still be part of natural history.

Schaffner's approach anyway deserves to be continued. His examples concerning the 'middle range' of biology are convincing, but one should be careful not to generalize his conclusions too readily, precisely because the issues at stake are empirical ones. An analysis of many more examples is needed in order to get the record straight. In later sections we will present additional examples of our own. One of them indicates that general laws for the middle range in biology are definitely possible (see Section 5).

Concerning Schaffner's description of integration in biology there is one additional point we want to make because of its importance, though it is largely outside the scope of this paper. His emphasis is on *description* of biology, and his description does show that (vertical) integration is common. But are all the integrative theories presented by biologists *adequate?* To answer this question one should move from descriptive to normative philosophy. We feel that this could drastically alter one's view of integration in biology. As argued elsewhere (van der Steen and Thung [1988] and van der Steen [1989]), many allegedly integrative theories of science actually represent pseudo-integrations.

Schaffner rightly argues that many discussions about laws in biology are one-sided because the emphasis has been on a limited area of biology. Evolutionary biology has received much attention, other areas of biology have been neglected. Moreover, we would add, the emphasis with respect to evolutionary biology has been almost exclusively on the neo-Darwinian paradigm (for alternatives in the making, see *e.g.* Ho and Fox [1988]; Weber *et al* [1988]).

Discussions concerning species and laws in biology, which we will evaluate critically in Section 4, represent a specific variant of this one-sidedness.

4 WHAT ROLE FOR SPECIES IN BIOLOGICAL THEORY?

Biology deals with organisms. Now organisms can be classified in many different ways, but there seems to be one basic type of classification, which is elaborated in taxonomy and systematics. Here organisms are classified into *taxa*, and taxa are put into a hierarchical system of *categories*. Individual species, as taxa, are put into the species category. At higher levels there are the categories of genus, family, order, etc. Most biologists (definitely not all of them) regard species as natural entities. On one definition of the species category, species are interbreeding entities, and interbreeding is regarded as a property which 'carves nature at its joints' (Mayr [1987]). The assumption that species are natural entities, however, need not imply that they are natural *kinds*. If natural kinds are characterized as entities with members sharing properties (a view which not everyone would accept), species are not natural kinds. There is much variability within species, so their names cannot be defined in terms of shared properties (see Beckner [1959]; his view on this matter is now commonly accepted). The prevailing opinion is now that particular species should be regarded as (natural) *individuals* from a logical point of view, but that the species *category* is a natural kind. However, there are dissenters. Ruse [1987] has argued that species are natural kinds of a sort rather than individuals. Other critics are Kitcher [1984] and Caplan and Bock [1988]. According to van der Steen and Voorzanger [1986], the issues have not been formulated in a sufficiently precise way; this may explain part of the disagreements.

Whatever stand one takes in the species-as-individuals issue, taxonomy (and more generally, biology) does not seem to have yielded general laws concerning particular species. So let us assume that there are no laws of this type. What consequences does this have? Two options for formulating laws and theories in biology appear to remain. Laws must deal with entities other than individual species, or they must mention species as a *category*. The issue has been discussed by various philosophers of biology. Here we will comment briefly on the views of Hull [1987] and Rosenberg [1985].

Hull deals with the second option. He is concerned with species being natural entities (individuals). Species have 'ontological status'. Hull argues that there must be laws which permit this conclusion because one needs laws and theories of science to make important ontological distinctions. What better way could there be to determine basic features of reality? Scientific theories should tell us which entities are real, and in what sense particular entities are real. Ontological status is theory-relative. We tend to agree. But we disagree with the consequences drawn by Hull.

> One consequence of this theory-relative view of ontological status is that species and organisms can count as individuals only if scientific laws can be found which range over them (p. 171). [That is, there must be laws which deal with the species *category*. And there seem to be such laws.] . . . As Ghiselin points out, Mayr's own claim that, under certain conditions, speciation occurs only allopatrically is an excellent candidate for an important and yet peculiarly biological law of nature (pp. 173–4).

This is the only law Hull mentions. It is formulated in a vague, qualitative way, so it is not a convincing example. Apart from this, we do not think that Hull's general argument has much force. Why should it be necessary for the species category to be covered by laws if species are to count as natural individuals, and the species category as a natural kind? A much weaker requirement to the effect that ontological status must be allotted to entities *on the basis of science* should suffice here. Science after all consists of much more than laws in a strict sense. (Arguments of Caplan and Bock [1988] against the species-as-individuals thesis could be construed as a defence of this thesis.) Perhaps a reconstruction of phylogeny (*i.e.* descriptive science which only involves laws in an indirect way) should lead to the conclusion that the tree of life consists of species as natural individuals, so that the category of species is a natural kind. It is not obvious that one needs laws ranging over species to reach such a conclusion. Moreover, the assumption that species are natural entities would not entail that species belong to *one* natural kind. All in all, Hull has failed to show that there should be laws mentioning species, if species are natural entities.

Rosenberg [1985] has a more convincing suggestion to the effect that basic laws of evolutionary theory could be formulated for species, as follows.

1. Species are lineages of descent among organisms.
2. There is an upper limit to the number of organisms in any generation of a species.
3. Each organism has a certain amount of fitness with respect to its particular environment.
4. If *D* is a physically or behaviourally homogeneous subclass of a species, and *D* is superior enough in fitness to the rest of the members of the species for sufficiently many generations, then the proportion of *D* in the species will increase.
5. In every generation of a species (not on the verge of extinction) there is a subclass, *D*, that is superior to the rest of the members of the species for long enough to ensure that *D* will increase relative to the species and will retain sufficient superiority to continue to increase, unless it comes to constitute all the living members of the whole species at some time (pp. 212–13.)

One might quarrel over the details of this reconstruction, but it indicates how laws concerning the species category could be formulated. The upshot of this section is that there cannot be laws about individual species, whereas laws about the species category are possible in principle. Hull's argument in favour of the latter possibility is unconvincing. Examples given by Rosenberg are better, although they are rather schematic.

Philosophers of biology at times suggest that possibilities for elaborating laws in *biology* depend on what one can say about species. That is very misleading. Those who reason in this way implicitly equate biology with taxonomy and/or evolutionary biology (subdisciplines which dominate present-day philosophy of biology). Even within evolutionary biology, species need not be at centre stage. After all, population genetics, according to many the core of evolutionary biology, is *population* genetics. When other areas of biology are considered explicitly, species are also sometimes misleadingly put at centre stage. Arguments in Rosenberg (1985) provide an example. He maintains that

> . . . there are in biology at most two bodies of statement that meet reasonable general criteria for being scientific theories. These will be the theory of natural selection and [perhaps] . . . general principles of molecular biology (p. 219).

Rosenberg offers the following argument designed to show that laws are impossible for the 'middle range' of biology (for comments on another argument he gives, see Section 3).

> . . . all those special branches of biology, ecology, physiology, anatomy, behavioral biology, embryology, developmental biology, and the study of genetics are not to be expected to produce general laws that manifest the required universality, generality, and exceptionlessness. For each of them is devoted to the study of mechanisms restricted to one or more organism or population or species or higher taxa and therefore is devoted to the study of a particular object and not to a kind of phenomenon to be found elsewhere in the universe. [In physics the situation is different because physics deals with] natural kinds, whose instances can and do occur throughout the galaxy. Particular species do not recur, and they cannot be expected to be much like particular lines of descent either elsewhere on the planet or elsewhere in the universe (p. 219).

We would not think that the disciplines Rosenberg mentions *are* devoted to the study of particular species (or organisms, populations, higher taxa). Why should physiologists be unable to formulate laws for the conduction of impulses in the nervous system which bypass idiosyncracies of particular taxa? The fact that nervous systems are absent in some taxa would not need to make any candidate-law non-universal ('if an organism has a nervous system, then . . .'). It is conceivable that Rosenberg's conclusion is correct, but his argument is not good. Anyway, it is profitable to leave taxonomy and evolutionary biology, the restricted domain of biology which philosophers often concentrate on, and move to other areas. This is what we will do in the next section.

5 INTERLUDE: SOME EXAMPLES

Temperature is one of the most important factors in the environment of animals. It affects chemical reactions and thereby has a marked influence on almost any biological process. One would expect there to be laws which express relations between temperature and the rate of biological processes, but as it turns out animals show many different kinds of reaction to temperature. Here we will consider reactions to extreme temperatures (for more details, see

Cossins and Bowler [1987]; some philosophical aspects were earlier discussed in van der Steen [1981]).

For a start, animals can adapt to extreme temperatures in two basically different ways. Some adaptations, called resistance adaptations, change the ability of animals to withstand lethal effects of extreme conditions; the rate of vital processes may remain unaffected in this case. Other adaptations, called capacity adaptations, modify the rate of vital biological processes so as to minimize negative effects of harsh conditions. We will briefly review capacity adaptations. Here one must again distinguish two basic categories, genetic and non-genetic adaptations.

Non-genetic adaptations involve changes in the performance of an individual during its lifetime. They often take the form of compensation, but there are exceptions. The 'normal' situation is as follows. If an organism is exposed, say, to cold, its rate functions will initially decrease, and this may affect performance in a negative way. After a while, however, compensatory reactions occur which restore the original rates. Such a secondary response is called acclimation. In ideal acclimation, original rates are indeed attained again. If the compensation is less than perfect, acclimation is called partial. There are also other possibilities. Compensation may take the form of an 'overshoot' (supraoptimal acclimation), or the response may be in the opposite direction (inverse or paradoxical acclimation).

How is all this to be explained? In many cases, responses can be understood as an adequate reaction to the ecological context. Predators which are exposed to cold will need energy for catching prey all the same, and will need more energy for keeping warm (if they are warm-blooded). So they had better improve on the rate of metabolism which is 'normal' for low temperatures. However, the result of this strategy may be disastrous when food is scarce. In that case a reduction of metabolism, perhaps accompanied by low body temperature and inactivity (*e.g.* hibernation) may be a better option.

Even in one ecological context, the options realized may be very different. There is a limit to possibilities for changing one's physiology. Some features of organisms will act as *constraints*, and the constraints need not be the same in different species. For example, some species may not have the equipment to withstand low body temperatures. Their option may be to mobilize energy by weight reduction under cold and food scarcity. And so on, and so forth.

The example shows that there are no *simple* general laws for rates of physiological processes as a function of temperature. Now it may be possible to formulate more complex laws which take account of exceptions, but there are limits to the complexity which is acceptable. Our impression is that one would have to formulate *very* complex generalizations to get rid of exceptions in the present example. As yet, the study of relations between the physiology of organisms and the thermal environment yields natural history rather than theory.

Our next example, animal growth, points in the opposite direction. We present it since it is outside the domain of candidate-laws which philosophers usually consider in commenting on theories of biology.

Young animals grow fast, adult animals may continue to grow for some time, but ultimately growth tends to stop. How is this to be explained? Some decades ago, the biologist von Bertalanffy [1938, 1969] developed an elegant theory to account for the phenomenon. It is reasonable to suppose that energy intake will roughly scale with body surface. However, energy expenditure, specifically expenditure of energy required for maintenance, will scale with body weight (or volume). Now, as organisms grow, surface will of course increase less rapidly than weight. So the relative amount of energy available for growth will decrease. As a consequence, the growth rate will gradually decrease, and growth will ultimately have to stop.

On the basis of these ideas, von Bertalanffy derived mathematical equations which should describe the growth of animals in the course of life. It was shown that many animals do show growth patterns as described by the equations. However, subsequent research revealed exceptions. So biologists began to use other mathematical equations to describe growth. One can always find an equation which describes one's results. The other equations work well in many cases, but unlike von Bertalanffy's equations, they are not supported by a neat candidate-theory.

So far the story resembles so many other stories of biological research. A 'law' is formulated, exceptions are found, and one ends up with natural history. But in the present case there seems to be a happy end. It is easy to explain the exceptions to the von Bertalanffy pattern. For example, if organisms are exposed to food shortage, von Bertalanffy's reasoning will simply not apply. Now it is possible to make his model more complex so that it accounts for items such as food shortage. This is what Kooijman [1988] has done. He has developed a model for energy budgets which is not *unduly* complex and which extends von Bertalanffy's reasoning. Kooijman was able to show that several hundreds of animal species from very different higher taxa exhibit growth patterns in accordance with the model. So far there are no convincing exceptions.

Whence the difference between the two examples? We wouldn't like to speculate about this. As far as we are concerned, the issue is an empirical one. In some respects (with respect to some variables), organisms apparently show a kind of variability which one cannot capture with laws of nature. With respect to other features, strict laws may apply. As the previous sections have shown, philosophers have a tendency to reason about such matters in an a priori fashion. Many of the arguments we analysed proved to be fallacious. *Biological* research will have to show what balance we can strike between biological theories and natural history. This is not to say that philosophy does not have its uses in this context. It can help biologists to develop strategies for discovering laws, it can put laws and theories in a good descriptive framework, and it can help to evaluate results of research. But it should not prophecy what the outcome of research will be.

6 UNIQUENESS, HISTORY AND LAWS

Many authors have argued in the past that the biological sciences have special problems because they deal with unique events: life on earth has a unique history and the evolution of any biological structure or process that we care to investigate was, at least in part, contingent upon unique sets of initial conditions.

In our opinion the problem of uniqueness *per se* is a red herring. *Every* event is unique in some respects. Explanations of unique events are traditionally given by specifying the relevant initial conditions besides laws in the explanatory premisses (see *e.g.* Ruse [1973], pp. 65–6). In this way, astronomers have long been able to explain and predict the occurrence of (historically unique) solar eclipses, for example. *Logically*, the situation is not different in biology.

In biology, however, uniqueness is more pervasive than in physics and chemistry because organisms are configurationally so complex that they can show much variability and idiosyncracy. In certain contexts idiosyncracies are precisely the issue of interest for the biologist, and it may turn out that this hampers the elaboration of laws and theories in biology *as a matter of empirical fact*.

Empirical matters are indeed at the core of a protracted discussion of uniqueness in the *Journal of Medical Philosophy* which started with an article by Gorovitz and MacIntyre [1976]. These authors have defended the strong thesis that philosophy of science is on a totally wrong track because it has failed to notice that we need sciences of *particulars*, biology and medicine being examples. Basically, their argument is that variability among organisms calls for a special approach which one does not need in physics. Generality needs to be de-emphasized in biology and medicine, and natural history is needed besides laws and theories. Papers by Schaffner [1980], [1986] have a place in the ensuing discussion. As we said in Section 3, we are in sympathy with the stance taken by Schaffner, which is more moderate than Gorovitz and MacIntyre's.

While particulars are indeed of interest in biology, variability need not prevent the biological scientist from abstracting general patterns within which many variations can be accommodated. To name but one example, Kluyver (see Kluyver and Donker [1926]) proposed that the myriad of bioenergetic pathways in living organisms all involve a series of coupled oxidations and reductions resulting from the inter- or intramolecular transfer of hydrogen. (Since then, electron transfer has come to be regarded as the basic event in oxido-reduction processes.) The detailed reactions of metabolism, aerobic or anaerobic, autotrophic or heterotrophic, are all covered by this general statement. The details are of profound interest, but the point is here that generalizations are also possible. Moreover, Kluyver's 'principle of the unity of biochemistry', as he called it, was of enormous heuristic value, especially in the field of microbial metabolism (see Collard [1976], p. 87, and Kamminga [1981]).

We would think that the situation is not fundamentally different with respect to historical aspects of biology, though there is the practical point that the reconstruction of initial conditions is likely to be more difficult in the case of history. However, various authors, most notably Gallie [1955] and Goudge [1961], have argued that particular episodes in the history of life need special kinds of explanation such as 'narrative explanation' which do not centre on laws. In the Devonian, the period when the amphibians originated, there were seasonal droughts. So individuals which developed legs were at an advantage. They could leave their habitat if it became too dry and so reach wet places again. Such is the flavour of narrative explanations. It has been argued later by various authors (*e.g.* Ruse [1973]), that this kind of reasoning is either totally unsatisfactory or it must covertly make use of laws. For example, the import of 'advantages' can only be understood in the context of natural selection and population genetics. It seems to us that this response is basically correct. Gallie and Goudge have failed to show that historical explanations constitute a special category.

Likewise, philosophers who have more recently argued in favour of historical explanations as a special category, have not provided a convincing account. Graham [1983] has presented a book-length defence of historical explanation, the main point being again that this form of explanation does not involve laws. He claims that a historical explanation of an event simply refers to (events in) its past. However, he does not give a precise characterization of the relations between *explanans* and *explanandum*.

Why the persistence of attempts to make a case for historical explanation? We do not think that these attempts are *wholly* beside the point. Biologists rightly wish to explain unique events, *i.e.* events which are described in relatively specific terms (not necessarily historical ones). In many cases such events are so *complex* (under the description which makes them unique) that a full-fledged explanation would require appeal to many laws and initial conditions. So, in practice, one has to be content with descriptive work or with 'explanation sketches'. In the latter case one will use laws, but these may stay in the background and they need not be formulated in a very accurate way. This is our own view of the matter in abstract terms (see also Hull [1975]). We will elaborate it by an example, the explanation of mass extinctions proposed by Stanley [1987].

The history of life is characterized by continuous processes of extinction and speciation. The rate of the processes, however, has shown marked fluctuations. Over and above continuous 'background' extinction there have been episodes of mass extinction on a global scale which affected a great variety of higher taxa, followed by periods of increased speciation. Mass extinctions obviously have a great impact on life on our planet, so it would be interesting to know their causes. In principle, the extinction of a species can be caused by a great variety of factors, *e.g.* 'normal' climatic ones, biological ones such as competition and predation, and extraneous factors like volcanism or meteors hitting the earth. So mass extinctions need not have a common cause. There is indeed much controversy about their explanation. Stanley [1987] has recently defended the thesis that all of them have been caused by the same factor, cooling on a global scale. Stanley's thesis that global cooling is all-important is by no means shared by all geologists, but we are not here concerned with its merits. Our aim is merely to analyse his line of reasoning because it shows that the standard conception of explanation on the basis of laws need not apply everywhere in science.

One of the factors which might be implicated in mass extinctions is changes in the sea level. Stanley reasons as follows about this factor (see *e.g.* pp. 36–40). When sea levels are high, continental shelves (areas of shallow sea adjacent to continents) are extended. As levels get lower, there is a vast reduction in shelf area. Now the number of species that can live in a particular region is known to depend on the area of the region. So numbers of species, at least of animals that inhabit shallow seas, should decrease as the sea level decreases. However, there are various reasons to reject explanations of mass extinction in terms of sea levels. Firstly, many of these extinction events also involved pelagic life far beyond the margin of continents. Secondly, some mass extinctions were not accompanied by changes in the sea level at all. In other words, a reduction in shelf area is neither necessary nor sufficient to bring about mass extinctions. But there is a stronger argument which involves laws, or at least empirical generalizations. The relations between diversity (species numbers) and area can be expressed by species-area curves. Now we have enough data to construct curves for terrestrial organisms, but it is more difficult to get at curves for marine organisms. We are not totally powerless, however. The data we do have show that life in some continental shelves with a restricted area is diverse. So diverse, in fact, that it is reasonable to suppose that reductions in species numbers (among animals inhabiting shallow seas) which are known to occur during mass extinctions cannot be explained by a reduction of continental shelf areas.

The latter argument may be reconstructed as follows. Data in our possession are compatible with many parameter values of species-area curves for animals in shallow seas. But no member of the *set* of admissible curves could be used for explaining mass extinctions. This shows that it is possible to reject a scientific explanation on good grounds even though the precise form of the 'law' it contains is not known.

Stanley follows a similar line of reasoning with respect to many other factors. He ultimately concludes, after an argumentation which one may characterize as inductive elimination, that global cooling is the best candidate to explain known mass extinctions. In addition, there is positive evidence pointing to the importance of this factor. The most important part of Stanley's argument consists in showing that the extinctions were in fact accompanied by global cooling. This amounts to uncovering initial conditions. What about the role of laws in the explanation? Stanley does not mention any law. But it is obvious that laws are presupposed here. Organisms have limited ranges of tolerance. So it is safe to assume that substantial, relatively rapid changes in some environmental factor should result in extinctions (if this is not a law, it surely is a sketch for a law). Would the temperature changes which accompanied mass extinctions have been marked enough to account for the phenomenon? That is a moot point. The inference is reasonable if one can show that the magnitude of the relevant changes was well beyond commonly observed ranges of tolerance, and that their rapidity would not permit genetic adaptation on a grand scale. Stanley's data show that this may well have been the case. The positive arguments for temperature as the main causative agent would be stronger if laws and initial conditions were given a more precise form, and that is hardly feasible. But the arguments *are* strengthened by negative ones showing that other factors can be excluded on reasonable grounds. By and large, the resulting pattern of explanation is in principle satisfactory.

So we end up with an interesting empirical generalization (itself *not* a law), that all the mass extinctions which occurred in the past have been caused by global cooling. The evidence contains all sorts of elements. There is much useful inductive reasoning. And laws play a role in the background, though they are not available in a very precise form.

In many respects, we would side with those who have rejected the thesis that historical explanations are special. Defenders of the thesis have not really articulated the concept of historical explanation. And some of the explanations they have offered are weak. However, the example we have given shows that theory and natural history have intermediates which play a role in explanation. In biology, the emphasis of research often is not on laws. But in many cases a weaker kind of general statement will suffice for perfectly sensible explanations.

7 DISCUSSION

In this article we have explored the factual situation that the biological sciences have comparatively few universal (even terrestrial-universal) statements of their own. Moreover, any general theories and laws that biology does have do not neatly yield lower-level statements without the addition of very elaborate and highly specific descriptions of initial and boundary conditions, about which more often than not only incomplete information is available.

Even if we accept that laws of nature, in any strict sense of the term, do not have as prominent a place in biology as in the physical sciences, it has been our intention to show that the possibility of formulating biological laws should not be dismissed too hastily. In particular, we suggest that it is first and foremost an empirical question whether laws ranging over properties at intermediate levels of biological organization are feasible. The two case studies discussed in Section 5 do not differ significantly from a methodological point of view—yet the outcomes as regards uncovering lawful regularities are quite different.

Moreover, we do not wish to attribute the lesser emphasis on general laws in biology to underdevelopment or methodological inadequacy of the subject. Even areas of biology where little appeal is made to laws, such as those discussed in Section 6, seem to us perfectly acceptable at the methodological level. This raises the question whether philosophers of science may have placed too much emphasis on general laws as a hallmark of good science. In addition, philosophers of biology may have concentrated overmuch on those areas of biology where one would expect there to be laws. Although our case studies in themselves do not permit sweeping statements about biology as a whole, we feel that they are fairly representative. By and large, laws receive little attention in many well-developed areas of biology.

There is no escaping the fact that biological systems (and their environments) are relatively complex and that this factor limits possibilities for developing general theories which allow explanation and prediction of behaviour in great detail. The emphasis is usually on antecedent conditions rather than on laws and the theories one encounters in the average biology text are almost always merely locally valid ones. We would regard the prevalence of such theories as a positive feature. In science one needs particulars besides generalities and in some contexts particulars may be very important, even if they are not covered explicitly by general laws.

How should philosophy deal with particulars, with natural history? We must admit that we ourselves have not provided a very precise characterization. Nor have we come across a satisfactory philosophy of natural history in the literature. Although the old ideals of logical positivism have few adherents nowadays (unified science, laws of nature as a hallmark of science, coherence only by reduction, etc.), they still survive in a covert way. Science is supposed to aim primarily at generality, and research which puts generalities in a secondary role is often deprecated. We would feel content if our analysis would stimulate some researchers to put the development of a philosophy of natural history higher on the agenda.

It is obvious that physics is more general a science than biology. We would not conclude from this that physics is fundamentally different from biology or that it better satisfies strict methodological standards. The arguments we have surveyed in this article do not, in our opinion, demonstrate conclusively that matters of principle are at stake. It is true, however, that the subject matter of biology is constituted by highly complex, variable systems of a kind that are, for most purposes, kept out of consideration in physics. Therefore, the fact that physics can keep at bay certain problems that confront biologists is at least in part a matter of convention. Here we would side with Miller [1987], who approaches the issue in a nice pragmatic way:

> The lesser rigor of historians' standard patterns as against physicists' and chemists', is largely the result of the greater freedom that physics and chemistry have to emphasize questions expected to yield approximately rigorous answers. ... when an explanatory question cannot be answered with something like a covering-law derivation, although the explanandum is due to a physical or chemical process, we locate that question, not in physics or chemistry, but in some other discipline, say, applied physics, applied chemistry, engineering, geology or biology (p. 133).

Given that we are interested in particulars as well as in generalities in many spheres, the need for a philosophy of natural history has relevance beyond the boundaries of biology. General theory and descriptive natural history are two ends of a broad spectrum which has analogues in other sciences. For example, in geology the two extremes would be represented by plate tectonics and detailed fieldwork, respectively. To which end of the spectrum the scientist's efforts should be directed depends on the nature of the problem under investigation.

However, there are those in biology who still want to make it more 'scientific' by a move towards the old paradigm of generality inspired by physics, narrowly conceived. Specifically, neo-Darwinian evolutionary biology is attacked because it concentrates on the historically contingent. The alternative (or supplementary) approach which is being developed is based on the following line of reasoning (for examples see various articles in Weber, Depew and Smith [1988]; also work by Goodwin, who aims at developing a 'structuralist' biology; see *e.g.* Goodwin [1984, 1988], Brière and Goodwin [1988]).

It can be shown by purely mathematical reasoning that relatively simple systems can develop quite complex behaviour even after slight perturbations if non-linear interactions are involved. This phenomenon is important for empirical research since it is exhibited by certain physical and chemical systems. On the basis of this, one can hypothesize that the evolution of biological complexity is due to similar phenomena. Logically, the number of possible structures and behavioural patterns that organisms could exhibit is so vast as to defy imagination. But the set of states that is actually possible in view of general structural, kinetic or energetic constraints defined by the properties of non-linear systems may be quite limited. This opens up the possibility of formulating laws of biology that make historically contingent matters less important. One can reach 'preferred' states by many routes depending on initial conditions. Preferences are important, idiosyncracies of trajectories are not.

This is admittedly a succinct summary which may come close to a caricature, but it captures the spirit of new approaches in biology. Would it be possible that the developments envisaged may lead to a new kind of biology with laws of nature in a more prominent position? We think that the question is misleading since the answer depends very much on how one chooses to delineate biology. True, if the promissory note concerning biological laws of the future would be actualized, there would be a change of emphasis. But such laws would have to concern a limited set of abstract features of organisms, defined in such a way that they are pervasive. More idiosyncratic features of organisms which now interest biologists would not thereby become less interesting. Of course one could decide to give the term 'biology' a narrower meaning which does not cover the natural history which now makes it a rich science. Then we would need a different label for areas defined away. But what's in a name?

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Literalism and the Applicability of Arithmetic

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ABSTRACT

Philosophers have recently expressed interest in accounting for the usefulness of mathematics to science.[1] However, it is certainly not a new concern. Putnam and Quine have each worked out an argument for the existence of mathematical objects from the indispensability of mathematics to science. Were Quine or Putnam to disregard the applicability of mathematics to science, he would not have had as strong a case for platonism. But I think there must be ways of parsing mathematical sentences which account for applicability of mathematics and also do not require us to believe in entities we have no evidence for, other than through reading these sentences literally. We will explore a particular way to interpret sentences of arithmetic which promises to account for their applicability without bringing in metaphysics not also brought in by science. The investigation will be limited to the arithmetic of cardinal numbers. The general strategy is to argue for the *analogy* between arithmetic and science, rather than to argue for one case having a particular characteristic independently of the other.

1 *The Putnam/Quine Account of Applicability*
2 *Mathematical Literalism*
3 *The Applicability of Arithmetic*
4 *Numbers as Natural Kinds*
5 *Beyond the Natural Numbers*

1 THE PUTNAM/QUINE ACCOUNT OF APPLICABILITY

Quine and Putnam have each independently argued for platonism on the grounds that only by invoking the existence of abstract objects would we be able to account for the applicability of mathematics to science. Putnam summarized the argument thus:

> [Q]uantification over mathematical entities is indispensable for science, both formal and physical; therefore we should accept such quantification; but this

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[1] Hartrey Field [1980] considered this to be the fundamental, although ignored, question in philosophy of mathematics. Philip Kitcher motivated his [1983] partly on the question of applications, and other examples are Stuart Shapiro [1983] and Mark Steiner [1989].

> commits us to accepting the existence of the mathematical entities in question Putnam [1971], p. 57).[2]

I question this inference; it is problematic to claim that the indispensability of quantified sentences of mathematics commits us to the existence of mathematical objects. There are a number of motivations for feeling uncomfortable with this inference. Perhaps the various motivations all involve a general discomfort with platonism, the view that there exist abstract entities; the issues are deep, however, and we will turn to some of them shortly.

There are two reasonable ways to block Putnam's inference. According to this argument, there are sentences of mathematics which are essential in some of the explanations and predictions of science. Some of these are existentially quantified mathematical sentences, whose range of variables includes mathematical objects. Examples of such sentences are 'There are 2 prime numbers between 6 and 12' and 'There exists a set which has no members.' These claims are relatively uncontroversial; somewhat less so, perhaps, is the assumption that physical and other scientific theories are true. The argument proceeds via the following two claims:

> (A) If physics is true, then the mathematical sentences used in physics, particularly the existential sentences, must also be true. In other words, the applicability of mathematics to physics implies the truth of the mathematical sentences used.
>
> (B) If the existential sentences of mathematics are true, then mathematical objects exist.

From (A) and (B) it is possible to reach Putnam's conclusion, that our scientific theories are committed to the existence of mathematical objects. On the standard account of truth, which includes Quine's criterion of ontological commitment, these sentences would not be true were there no mathematical objects satisfying the descriptions given. In this paper, we will explore some of the consequences of giving up (B). In passing, however, I would like to note that the other strategy has been attempted. This strategy is to give up (A), that is, to deny that the applicability of mathematics requires that sentences of mathematics are true. Notably, Hartrey Field [1980] attempted to show that applicability of mathematics does not imply the truth of mathematical sentences.[3] His strategy is to combine a proof of the conservativeness of mathematics over scientific language with a re-parsing of scientific language into language which is not committed to abstract objects. Although Field's particular attempt has faced some difficulties, it is not conclusive that the program in general is doomed. That is an issue for another occasion.

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[2] Similarly, Quine wrote: 'The reason for admitting numbers as objects is precisely their efficacy in organizing and expediting the sciences' ([1960], No. 48, p. 237).

[3] There are several obstacles to the success of Field's program. One of the most central of these is discussed by Shapiro [1985].

Whereas denying (A) would block the move from applicability to truth, denying (B) blocks the move from truth to ontological commitment. For to deny that (B) holds is to deny that the truth of existential sentences in mathematics commits us to the existence of mathematical objects. The move from truth to ontology involves two presuppositions. One is the correctness of Quine's criterion of ontological commitment. Quine argued that the ontological commitment of a given theory is entirely determined by what must exist in order for the existential sentences of a theory to be true. As Quine put it,

> we are convinced of a particular ontological presupposition if, and only if, the alleged presuppositum has to be reckoned among the entities over which our variables range in order to render one of our affirmations true [1953, p. 13].

Quine's criterion has drawn fire from various directions, and there are certainly reasons to inquire into whether it should be used generally. In this paper, we will not question this presupposition.

The other presupposition behind premise (B) is that mathematical sentences should be taken at face value. I call this presupposition *literalism* in mathematics. Literalism, when taken together with Quine's criterion of ontological commitment, says that a theory with existentially-quantified sentences is committed to a domain in which occur entities of the appropriate type to satisfy those sentences. In this paper we will explore some of the consequences of giving up literalism in the attempt to come up with an alternative account of the applicability of mathematics.

2 MATHEMATICAL LITERALISM

There is a tradition of reading sentences of our mathematical theories literally. Not only might we be predisposed to read sentences in our theories at face value, but this inclination is fueled by influential theories which do so as well. Russell and Whitehead's *Principia Mathematica* had a very great influence on the development of the philosophy of mathematics in the 20th century. One the many ways in which this three-volumed work influenced the foundations of arithmetic, in particular, was to treat names and definite descriptions of arithmetic as picking out objects, in particular, classes. Whitehead and Russell defined the cardinal numbers as classes of similar classes:

> The Cardinal Number of a class α, which we will denote by "Nc'α," is defined as the class of all classes similar to α, *i.e.* as $\hat{\beta}$ (β sm α) [1910–13, p. 4].

The definition originated with Frege, who argued for it 30 years earlier.[4] There were reasons, which we will not go into here, for regarding mathematical entities as objects, but these can be questioned. I have argued in [1988a] that Frege's reasons for his version of literalism did not force him to conclude that mathematical entities are objects. Like Frege, Whitehead and Russell defined the number 0 as the class of all classes similar to the class $\hat{x}\ (x \neq x)$, that is, the class of all empty classes. Since there is just one of these, the number 1 can be defined as the class of all classes similar to the number 0. The tradition set by Frege and Russell and Whitehead was not limited to the definition of the cardinal numbers. In general, all mathematical entities were defined in set-theoretic terms, so that every object of our mathematical theories was to acquire the status of an 'object' in the form of a set or a class. This was to be true of mathematical properties, relations and functions as well, as these were defined by means of their extensions. Moreover, many sentences in our mathematical theories contain names and definite descriptions ostensibly referring to mathematical entities. A common way of interpreting these is to use Russell's theory of descriptions, resulting in more existentially quantified sentences which, once again, are committed to mathematical entities.[5] To read these at face value would be to hold that numerals ('3') and other names ('The Fibonacci series') name objects, and to hold that definite descriptions ('the union of the empty set with its singleton'), when well-defined, refer uniquely to objects satisfying those descriptions.

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[4] See Frege [1884], Sect. 68 and [1893]. After Frege's system was found to be inconsistent, Whitehead and Russell constructed their type-theoretic system which was supposed to carry out the intent of Frege without the same failing.

There is another sort of justification for literalism which stems from a desire for overall unification of theory. We desire to interpret mathematical language the same way we interpret scientific language until we have a good reason to do otherwise. This was expressed by Paul Benacerraf:

> An account of knowledge that *seems* to work for certain empirical propositions about medium-sized physical objects but which fails to account for more theoretical knowledge is unsatisfactory—. . . A theory of truth for the language we . . . mathematize in, etc., should by the same token provide similar truth conditions for similar sentences. . . . Any departure from a theory thus homogeneous would have to be strongly motivated to be worth considering (Benacerraf [1973], p. 662).

In the absence of overriding reasons to the contrary, we ought to interpret mathematical language in the same way we interpret language in other domains. Thus, to the extent that we read identity statements and subject-predicate sentences at face value, so also ought we interpret their analogues in mathematics. This argument places the burden of proof on the side of the anti-literalist. There is a general advantage in having a homogeneous semantic theory which applies to all areas of discourse including the mathematical. We interpret names and definite descriptions in physical theories as picking out objects, so, other things being equal, we ought to do so in mathematical theories as well.

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[5] Russell [1905]. Names can be parsed into definite descriptions in the manner of Quine [1953] so that, for example, x is Pegasus becomes x is the thing that pegasizes.

This would constitute sufficient reason were other things equal. But are they? There are areas of language in which our semantic theory is not unquestionably the same as the most straightforward Tarskian referential reference to objects. Consider our use of language to communicate fictional narrative; there may be a way of treating fiction in a semantically homogeneous way as other discourse, but this is certainly not obvious nor straightforward. Moreover, whether or not there are many areas of our language which ought not to be treated at face value, there are certainly reasons to think our mathematical discourse is different from that of other areas. One is that mathematical objects are multiply instantiable in a way that it is not at all clear that trees and people are. This point was most forcefully made by Benacerraf [1965] in his paper 'What numbers could not be'. In this paper, he noted that the semantic role numerals and definite descriptions play in a formalized theory of arithmetic is quite different from that of names and definite descriptions about physical objects in biology or physics. It is problematic to ask for the reference of a term in a formalized theory, because the theory does not have a unique interpretation; many models can satisfy all the sentences so that the theory itself has no resources to make a choice between them. For example, choosing between set-theoretic models of formal number theory cannot be determined by number theory alone and so there is a sense in which whether a given set is the reference of a given term is arithmetically irrelevant. If the nature of a particular number is entirely given by number theory, numerals do not have the semantic representation of names and definite descriptions in other sciences: they do not refer *simpliciter*. Benacerraf therefore suggested that we give a non-literal reading of sentences of arithmetic, something closer to the idea that numerals be regarded as predicates describing *criterial conditions* to be satisfied by some object or other. For example, the number 3 is a set of conditions an object must satisfy in order to play the role of 3 in a progression. It might seem natural to conclude that numerals are *predicates* which apply to objects in the models of number theory.[6] The fact that numbers appear to lack the sort of robustness that trees and people have takes some of the strength away from the homogeneity requirement for arithmetic.

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[6] Benacerraf did not suggest this. It would have been problematic given the extensional interpretation of predicates in the area of the foundations of mathematics. To take this course would, in effect, be to say that numbers are sets, against Benacerraf's original conclusion. Moreover, problems of consistency arise. If we take the numeral '3' as a predicate, corresponding to the set of conditions something must satisfy in order to be the *third* in a progression, and then interpret it extensionally, the number 3 turns out to be the universal set, for everything occurs in third place in some progression or other. But worse, *each* number would turn out to be the universal set.

It is thus not straightforward to say that the thesis of semantic homogeneity carries over into the case of mathematics. Ironically, however, we will come back to this very homogeneity thesis by a different route. For I will argue for a view below which gives the sentences of science and arithmetic similar semantical treatment. However, this will be homogeneity without literalism, for literalism would block a natural picture of the applicability of elementary parts of mathematics. A literal interpretation of the sentences of our mathematical theories leaves the applicability of these theories very unclear. To give an account of the applicability of arithmetic is to explain what happens when we use concepts, methods or results of arithmetic in the process of scientific discovery or explanation. Literalistic theories of arithmetic can account for its applicability, but only implausibly. For example, theories, like Whitehead's and Russell's program, according to which numbers are sets constructed from axiomatized set theory, must resort to some degree of set-theoretical apparatus to extract applications to the world. The resulting picture is not as straightforward and depends on a theory whose objects, sets, may not exist in the world science seeks to describe. I will propose an alternative view which, I believe, gives a natural and plausible account of the applicability of arithmetic to the world and in science. In this view, numerals are to be interpreted as predicates which apply to aggregates, yielding statements about the cardinality of the aggregates. The difficulty with regarding terms in mathematics as genuinely referring points in this direction. And the promise of a natural account of the applicability of mathematics points in this direction as well.

3 THE APPLICABILITY OF ARITHMETIC

A century and a half ago John Stuart Mill sketched an empiricist philosophy of arithmetic according to which arithmetical laws and numerical formulae are laws of nature (Mill [1843], III, xxiv, pp. 2–9). Mill's view was attacked by Frege a few decades later, and partly as a result of these criticisms, Mill's view was underrated for a long time (Frege [1884], Sects. 7–10, 16–17, 22–25). There is much that is good in it. Two features of Mill's view are the most interesting and the most fruitful: (1) One is to regard numbers as *properties of aggregates* of things counted. This contrasts with the idea that numbers are themselves objects. The multiple instantiability of mathematical objects is evidence for this. Also, we can easily imagine our predicating number of groups of things, rather than finding a relation which has for its relata on the one hand, a group of things counted, and on the other hand, an object, that is, the number. It also contrasts with any other structural variation, for example, where a number is a relation. (2) The second is the view that numerical formulae, like '$2+2=4$', are *laws of nature*. This contrasts with various views which put arithmetic outside the realm of the natural sciences on the grounds, for example, that they are necessary truths, or that they are a priori knowable. I think these ostensible differences are at best a matter of degree. By contrast, interpreting arithmetic as not different in kind from biology or physics according to these two Millian theses suggests a natural and plausible account of the applicability of elementary arithmetic. Indeed, truths of arithmetic resemble a certain kind of truths of natural science surprisingly closely, and numerals often function like ordinary predicates.

Another recent neo-Millian philosophy of arithmetic was motivated by the desire to defend Mill against Frege's attacks. Glenn Kessler proposed that numbers are relations so that

> in claiming that a certain aggregate x contains 52 cards we are claiming that the numerical relation 52 obtains between the aggregate x and the property of being a card. We might write this as '52 (x, being a card)' (Kessler [1980]).[7]

Kessler re-wrote Peano arithmetic in such a way that his analogues to the number-theoretic theorems can be derived. There are a few technical difficulties (see Simons [1982]). But more important, even if the view can be made 'formally correct' in the sense that a model for arithmetic is supplied which satisfies certain desiderata, it seems somewhat artificial: each numerical application will involve a property ('being a card') and a relation ('52 (x, y)') whose ontological status must be established. The proposal I sketch here has it that when I count cows in the field I am subsuming the group of cows I see before me under the property of being three-in-number. Kessler's proposal has it that I would be asserting that the property of being a cow in the field is related by three-ness to the heap of matter I see before me. Although I think the view I develop here is more natural, in the final analysis, the differences between Kessler's view and mine might be quite superficial: both involve ontological commitment to universals of some kind (depending on background theory of predication), and so find complex structure in the natural world. Kessler suggested that his view could be, and ought to be, shown to be coherent with the fact that numerals seem to function much like quantifiers. Therefore we might regard the present proposal not so much as a rival account to Kessler's, but a development of an important idea which any view should also be consistent with.

To give an account of the applicability of arithmetic is to explain what happens when we use some concepts, calculations, theorems or laws of arithmetic in an essential way to reach a conclusion about something in science or in the world. Arithmetic is used to obtain information and to provide explanations. If we have an account of the applicability of laws of nature, it would suffice to show that theorems of arithmetic and laws of nature play the same type of role in a scientific theory. For this would be to show how the arithmetical laws can be used to predict and explain in the same way laws of nature can. As a step toward a more comprehensive theory, I will describe a model of applications of laws and then show that the application of cardinality assertions fits that same model. We will only look at the most simple parts of arithmetic, and will concentrate on the 'covering-law model' of explanation as a rough guide for paradigmatic cases of applicability of laws.

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[7] Related proposals include Kitcher [1983] and Maddy [1980].

The covering-law model of explanation is best known in the classic paper by Hempel and Oppenheim [1949]. On this model, a scientific explanation is an argument whose conclusion is the explanandum, and the set of whose premises form the explanans. Every premise and the explanandum must be true. At least one law must be in the explanans, which may or may not be statistical, and there may possibly be descriptions of some initial conditions. On the Deductive-Nomological model, an explanation is a valid deductive argument; on the Inductive-Statistical model, an explanation is a strong inductive argument whose explanans provides strong grounds for expecting the occurrence of the explanandum. On the traditional view, predication is a sort of converse of explanation. When an explanation is given, we start with the explanandum and look for laws and, possibly, initial conditions, to insert as premises to construct an argument from which the explanandum follows. When a prediction is given, we start with the premises of an argument, usually one or more laws and initial conditions, and derive the conclusion, either by logic or calculation. Applying a law of a particular scientific theory is using it as one component of a process that furthers the aims of the practice of that science. This typically involves either giving an explanation for some phenomenon, or making a prediction. In cases where the law is being used to control a situation, this can only be done by means of some intermediary predictions. Using the covering-law model as our guide, we might say an *application* of a law is the construction of an argument of which the law is a premise, and the conclusion is the explanandum or the desired prediction. In the simplest of these cases, the sentence expressing the law will either itself be a generalization, or entail a generalization. The explanandum will be an instance of it.

To illustrate the general idea, suppose the following is true except perhaps for deviant cases:

1. Whales breathe air.

Think of this for the moment as a 'lawlike' claim. From this statement, and my knowledge that Moby Dick is a whale, I can conclude (or predict) that Moby Dick breathes air. I am *applying* this lawlike claim to a particular case. In the process I am interpreting the lawlike sentence [1] as expressing or entailing a generalization about biological organisms:

2. For any organism x, if x is a whale, then x breathes air.

And I instantiate by putting in 'Moby' for x. I purposely did not write my lawlike claim [1] as a generalization, because, as we will see later, there are reasons to think that may not be the correct representation. Be that as it may, it must entail a general claim which I instantiate to a particular instance.

In some applications of arithmetic, theorems about numbers are used analogously to the way in which the law-like claim [1] was used above. If I know that Mars has 2 moons, and Neptune has 3, I can infer that Mars has fewer moons than Neptune, and that together there are 5. Pre-theoretically, these inferences seem obvious, but it is instructive to see clearly the grounds on which such inferences are justified. To conclude that Mars has fewer moons than Neptune, I must be appealing to a belief that whatever is 2 in number is fewer in number than anything which is 3 in number. In our formal theories of arithmetic, we have a theorem which is relevant, in some way, to the belief I appealed to:

3. $2<3$.

Since I used the theorem to reach a conclusion about aggregates of moons it seems that [3] is elliptical for, or entails, a claim about aggregates of moons. For if I applied this theorem to the case involving moons I was assuming that it entitled me to assert something like the following:

4. For any aggregates x and y, if x has 2 elements and y has 3, then x has fewer elements than y.

And I instantiated by putting in 'the aggregate of moons of Mars' for x and 'the aggregate of moons of Neptune' for y. A fact about the physical world was inferred using a theorem of arithmetic as a premise. In effect, my conclusion that Mars has fewer moons than Neptune, is a prediction. The premises of the argument include the 'lawlike' claim [3] that $2<3$, and statements of the particular cardinalities of the planets. So this example of an application of arithmetic can be put into a Hempelian model of prediction.

The generalization [4] introduces a new relation 'has fewer elements than' which should be defined in terms of a relation of elementhood for aggregates. We will for our purposes at present let this relation correspond to a rough and ready notion given by our intuitive idea of an aggregate as a group of things such that one of those things is an element of the aggregate. Although I will not go into detail here about the ontological status of aggregates it should be noted that there has been considerable worry about the nature of aggregates on a Millian view of arithmetic. One might at first think that we could import the notion of set from an axiomatic set theory like *ZF*. A general problem with this is that in order to have a plausible theory of applicability to science aggregates ought not to be stipulated by a priori or arbitrary theorization, but rather determined by the science in question. Moreover, if one of the motivations of our position is to avoid metaphysical commitment not already borne by science, we ought not to be forced to drag in sets.[8]

Simple theorems without quantification like the one above can in general be interpreted to fit this extension of the covering-law model. For example, the law-like sum $2+3=5$ will be interpreted as a law-like claim from which I can infer a generalization about aggregates. This generalization may take the form: For any two aggregates x and y, if x is 2-in-number and y is 3-in-number, then the aggregate which results by putting them together is 5-in-number. Again, we have some unexplained relations, like 'putting together' which would have to be defined in terms of the elements of which the aggregates consist. This will require a claim that the union of two given aggregates exists, a version of the Axiom of Unions for aggregates. Alternatively, the generalization itself may assert the existence of the aggregate which results from 'putting together' the first two. Not surprisingly, the more complex the underlying arithmetic, the more complex the corresponding generalization would have to be. There will be a difference of kind, however, in the case of sentences which quantify over numbers; these will be discussed later.

Our examples have involved cases where the arithmetical theorem is applied in order to make a *prediction*. Arithmetic can also be applied in the process of giving *explanations*. While the covering-law model may be a fairly accurate picture of typical cases of prediction, as a model of scientific explanation it has since been superseded by more precise accounts.[9] Not all arguments are explanations, and most likely not all explanations are arguments. By contrast, a detailed articulation of the pragmatic theory of explanation, along the lines of van Fraassen's model in particular, may well in the end provide the better framework for the scientific explanation and, by extension, for the applicability of mathematics (van Fraassen [1980] pp. 134–58).[10] The pragmatic theory of explanation holds that whether a given description is the right one depends on contextual features such as the interests and background knowledge of the one seeking the explanation. We can see why the greater generality of the pragmatic theory is a good idea in the case of applications of arithmetic where an explanation is desired. Usually, when an explanation is desired, the desired explanans will not be a simple arithmetical fact but rather a description telling why a certain arithmetical fact is relevant or interesting in the case. Suppose I wanted to know why the second grade class has fewer children than the third grade class. A simple covering-law model would construct an argument in which the explanans includes initial conditions: the second grade has 10 children, the third grade has 14 children; and also the arithmetical theorem: $10 < 14$, and so the conclusion, our explanandum, the second grade class is fewer in number than the third grade class. This argument would rarely be the desired explanation. We would most likely be seeking some reason why there are more students in the third grade, not why the number of children which the second grade (actually) has is smaller than the number the third grade (actually) has! But there will be some cases where the arithmetical theorem provides the desired explanation when the arithmetical fact is particularly obscure or the person seeking an explanation is ignorant of the relevant fact. We might explain to a child why there are not enough cookies to go around for each person to have 2 cookies, if there are 4 people and 6 cookies, by saying that 2 times 4 is 8 and 6 is less than 8. Here the theorems $2 \times 4 = 8$ and $6 < 8$ are being used in the place of laws in a Hempelian explanation for the explanandum. Of course this is just one of many possible desired explanations for that explanandum (consider: because the dog ate the rest of them), indicating that a given explanandum almost never, if ever at all, has a unique explanation. Although there is a need for a pragmatic model of explanation in general for science and arithmetic, many actual explanations, once the contextual variables are filled in, do fit the covering-law model. This simple model serves as a very good outline, usable as long as we keep in mind the fact that the classes of explanations and arguments fitting Hempel's expectations are not co-extensive. Our use of it here, to indicate a way of seeing simple parts of arithmetic fitting into the practice of science by means of providing some of the very laws, or at least law-like generalizations, may serve to point in a direction in which to develop a more comprehensive picture.

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[8] The best-known problems were introduced by Frege in his criticism's of Mill's view (Frege [1884], §§ 22–24, §§ 46–8). (1) The generality argument states that arithmetic is much more general than aggregates would allow; one can collect together in one's thought and count entirely unrelated and dispersed groups of objects. (2) The relativity argument states that aggregates must come already given a cardinal number in order to count them, for the same heap of matter can be carved up in many ways. We do not want to have to say that the one copse of five trees has both the number 5 and the number 1, so number must apply to something other than the aggregates. The names of these arguments is from Kessler [1980].

Both of these arguments impose on arithmetic a condition that natural science itself does not satisfy. They both use the fact that the range of desired applicability of arithmetic is broader than the stock of elegible aggregates. But in the natural sciences we often attribute scientific magnitudes and characteristics, like natural kinds, to things which are not themselves real objects of science. The northern half of my house might not be a real object of science. Nevertheless I can meaningfully attribute to it a determinate mass and chemical composition. The Fregean arguments can be disarmed by the strategy of pressing the case of natural science on to the arithmetic case, and the analogies hold surprisingly well. By contrast, Frege was not engaged in assimilating arithmetic to science and could not avail himself of that strategy.

I have also addressed some related objections to this proposal in my [1988b].

[9] One problem with it is that it is too broad: not every argument is an explanation. Consider the well-known example, introduced by Wesley Salmon, that according to Hempel's model we ought to be able to explain why a certain man did not become pregnant by appealing to the fact that he took birth-control pills (Salmon [1971], pp. 33–4).

It is arguable as well that Hempel's model is too narrow. Often we are satisfied with an explanation which does not increase the expectancy of the explanandum. Salmon had an example for this as well: we can explain a person's getting paresis by appeal to the fact that the person had latent syphilis, even though most people who have syphyllis do not get paresis (p. 56). While this case might be considered merely elliptical quantum mechanics may provide examples of genuine explanations which do not consist of sound arguments of any kind.

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[10] See Garfinkle [1981] for a version motivated from needs in the philosophy of the social sciences.

4 NUMBERS AS NATURAL KINDS

We have seen an analogy between the use of a certain type of law-like truths of science and certain simple arithmetical theorems. There is more to the analogy. The arithmetical truth that 2 is less than 3 says something stronger than merely the generalization that all 2-membered things are fewer in number than 3-membered things. The statement that 2 is less than 3 does not just apply to actual 2-membered aggregates, but reaches beyond to anything that might be 2 or 3 in number. On the basis of the fact that 2 is less than 3, I can assert that if Earth had 3 moons, then Mars would have fewer moons than Earth. The generalization does not cover this case, for Earth does not satisfy the antecedent. And clearly we make such inferences. In the old cliché from philosophy of science, truths of arithmetic 'support counterfactuals' in the same way in which law-like statements are said to 'support counterfactuals'. Above we described [1] (Whales breathe air) as a law-like statement. If it is part of the nature of being a whale that it breathes air, then I am entitled to infer that if my catfish were a whale, it would also breathe air.

I have so far left it open whether our paradigmatic law-like sentences [1] and [3] should be interpreted as *expressing* their corresponding generalizations, or as merely *implying* them. The issue is whether such statements express something stronger than just that the claim holds generally in the actual world. The above considerations involving modality indicate that the sentences in question are properly stronger in expressed content than their corresponding generalizations. For if such a statement entails conclusions about non-actual situations, it seems that it cannot be merely expressing a generalization about actual things. Indeed, a recent theory of laws of nature seems especially tailored to scientific truths of this kind. This theory was introduced independently by David Armstrong [1978] and [1983], Fred Dretske [1977] and Michael Tooley [1977]. On this view, laws are facts about universals. I call this the Property Theory of Laws. Each of these authors defended the thesis that statements of law are best interpreted as singular statements about universals. This interpretation is to hold even if their grammatical form indicates that they are generalizations about particular things. For example, the law-like statement [1] (Whales breathe air) is to be interpreted as expressing the fact that a certain kind of relation holds between *being-a-whale* and *breathing air*. According to this view [1] might be expressed as

[1*] Being-a-whale ⇒ Breathing-air.

On this view, the law-like statement is to be interpreted as being about two scientific properties, or universals, saying that a certain relation holds between them. What this relation is will vary from law to law, and it is not clear what it is supposed to be. Presumably this is a job for the particular science in question, not for a priori philosophizing. As a stand-in, Armstrong introduced the higher-level relation of Necessitation in his expressions of statements of law. His representation of our law statement [1] would be something like 'Being-a-whale *Necessitates* breathing-air'. I follow Dretske's use of a double arrow to avoid pre-judgement of the relation in question. But these are merely notational variants for a higher-level relation that has yet to see formalization.

My examples, like [1], are scientific truths that are law-like in that they support counterfactuals and are, like many laws of the special sciences, exceptionless except for abnormal cases.[11] I think of these scientific properties as *natural kinds* which I only characterize roughly as, on a realist presupposition that there exists such a thing, the real kinds that the world is divided up into independently of our theories and knowledge.[12] Examples of putative natural kinds are gold, lemons and the species *magnolia grandiflora*. Of course there are deep problems related with the notion of a natural kind. There is the question of whether it is meaningful or correct to think of kinds as having essences, so that something is a member of a kind just in case it has the properties constituting the essence. A related problem extends from the taxonomist's problems with drawing a line of demarcation between one species and another; although reproductive isolation is a guiding factor, morphology is also used, resulting occasionally in some arbitrary decisions. Although I will use the term 'natural kind', the present thesis does not depend on the notion of natural kind being unproblematic. In particular, the present thesis only requires that there be scientific truths which support counterfactuals.

A qualification about the concept of law is also in order. In our application of the Property Theory of Law, I want to be able to use the term 'law' quite loosely. Most of today's successful candidates for laws of nature are equations between magnitudes, like the Hardy–Weinberg equilibrium or Coulomb's Law. The proponents of the property theory of laws have generally used illustrations involving law-like statements of the form All A's are B. While equational statements of law might be representable in that form, there are various kinds of laws which play a variety of roles in explanation and prediction. The thesis advanced here does not depend on the claim that the universal truths are laws of nature. We may embrace the view that laws involve universals in order to use the idea for our kind of case. We need not thereby commit ourselves on the question of their being *laws*. And indeed, the proponents of that theory had better not insist that their view depends on laws being of the form All A's are B, lest they leave their view needlessly open to criticism. The important issue is that the scientific truths we are looking at support their counterfactuals. Only for simplicity of expression do I use the word 'law' for these cases.

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[11] It is important to allow for exceptions to laws in most sciences. For example, consider law-like regularities which attribute a certain pigmentation to members of a certain species. (The old example is 'Ravens are black'.) The regularity would be law-like in the sense that it supports counterfactuals and can be used in prediction and explanation in a way an accidental generalization cannot. Nevertheless, there are albinos, and these are exceptions to the law. Jerry Fodor [1974] addressed the fact that laws can have exceptions.

[12] The concept of natural kind has gained recent fame through the semantics of natural kind terms. See, for example, Putnam [1973].

The Property Theory of Law adds a wrinkle to our earlier covering-law model of the applications of laws. Above we left it open whether the law acting as a premise in the explanans expressed the generalization from which an instance is made. The Property Theory of Law makes it very clear that the law is logically stronger than the generalization. The explanans, then, must include both a statement of the law in question, and the appropriate generalization. But a complete argument will also have a connection between the statement of law and the generalization. So the Property Theory of Laws must presuppose an inference schema which licences inferences from law to generalization. The schema should take us from statements asserting relations to hold between universals to statements asserting that other relations hold between things having those universals. The inference schema would have to be instantiated for the case of a particular law. In the case of [1] it might be:

5. If Being-a-whale $\Rightarrow$ breathing-air, then, (x) (x is a whale $\rightarrow x$ breathes air).

This removes the law an extra step away from the actual phenomena explained or predicted.

The same wrinkle occurs in the applications of arithmetical theorems. The theorem [3] $(2<3)$ is interpreted as expressing an assertion involving a relation between two universals, being 2 in number, and being 3 in number. If we have an inference schema sufficient to handle a variety of applications outside of arithmetic, it ought to extend to arithmetic as well. The rule of inference to use in our example would then be of the following form:

6. If $2<3$, then, (x) (y) ($2x$ & $3y \rightarrow x$ is fewer in number than y)

This rule of inference will play an essential role in the application of the theorem [3] to planets and moons. If the application is the prediction that Mars has fewer moons than Neptune, the first stage involves getting from the theorem [3] to an appropriate generalization. On the covering-law model, the explanans will include the law-like [3] and the rule of inference [6] to get me to the appropriate generalization. In the second stage I have included the generalization [4] into the explanans, and then I instantiate the right planets into the generalization.

The picture of application I have in mind treats numbers as universals and this might seem to detract from the plausibility of the natural kinds picture of arithmetic. Recent versions of realism in mathematics have regarded numbers as objects. At best, it might seem that, while the present proposal avoids commitment to numbers as abstract objects, this is no gain since the numbers have merely been replaced by universals. And indeed, one might well prefer the traditional abstract objects to universals on the grounds that universals have unclear identity conditions. It is therefore important to see that there are good reasons to engage in the ascent to universals. The essential feature of the Property Theory of Laws, and our use of it here, is that it shifts the locus of reference from objects instantiating universals, to the universals themselves. The original proponents of the property theory offered a variety of arguments in defense of their theory, and I refer you to their published works. I want to bring out here one further virtue, independent of its applicability to arithmetic. Suppose that it is a law that whales breathe air. On this assumption, it is impossible for there to be whales that do not breathe air. What would make this true? I think there is only one answer here: there is some genetic material the presence of which is necessary for being a whale and sufficient for breathing air. Of course, this entails that there are necessary conditions for being a whale. If this is false, that is, if there is no condition which is necessary for being a whale, then, perhaps, there is no such thing as a law involving whales. The problem is that it is generally recognized today that biological species are variable in the sense that the traits of typical individuals vary over generations. It is problematic, therefore, to assume that there are necessary conditions for inclusion within a species. Perhaps this indicates that there are, strictly speaking, no laws of this kind. However, we can talk about necessary conditions for inclusion within a species as it now occurs. For clearly there is continuity enough in biological natural kinds to satisfy our intuitions that 'Whales breathe air' is a law-like statement. With this caveat in mind, we might then interpret our statement about whales as expressing a fact about genes, maybe:

7. The genes necessary for being a whale also code for the ability to breathe air.

As it stands, this sentence does not succeed at answering the 'supporting counterfactuals' objection to extensional theories of laws. For it could be interpreted as expressing a generalization about the genes of this whale and the genes of that whale, but with no reference to genes of non-actual whales. In order to avoid succumbing to the same objection, we must interpret that sentence as expressing a fact about a genotype:

8. The genotype for whales codes for the ability to breathe air.

By moving from gene to genotype, we have engaged in 'ontological ascent', committing ourselves to universals shared in common by many genes or sets of genes. We trade possible worlds for universals. The gain is that we have put the modal facts back into the actual world. While the possible world apparatus has been a convenient fiction for talking about modality, we can be clearer about the price of our modal realism by keeping the modal ontology in the actual world.

The move to universals might be parsimonious as well. Traditionally, universals have been thought to be exactly the sort of thing which results from profligate ontology. Universals were victims of Ockam's original razor. But the Principle of Parsimony does not enjoin us merely to limit the number of entities or the number of types of entity, although that is one possible result of parsimoniousness. It applies more generally to the notion of an explanatory strategy, and to the explanatory power of a whole theory.[13] Numbering entities or types of entities are special cases of the more general injunction. An explanatory strategy which treats a large number of diverse phenomena in the same way may be very powerful. It might introduce a new entity or type of entity, and still be more parsimonious than any alternatives. A theory which lists one demon engaging in human activities will explain much less than a theory which posits intangible forces and strange invisible particles with new unheard-of characteristics, if it explains a great part of our experience with a unified story line. In short, the Principle of Parsimony does not absolutely rule out universals *simpliciter*. If their introduction streamlines theory, serves to bring parts of mathematics under the same explanatory strategies as much of science, then it may well be parsimonious. The objections involving ontological commitment are interesting and well-motivated, for it is a subtle issue to exchange abstract objects of one sort for another. But universals may have the advantage here once we place the case of arithmetic in a larger context of applications in science, for there are in general overriding considerations to conform one's ontological apparatus to that of the larger theory.

It is, however, possible to accept the main points of the present analogy between arithmetic and science while not taking on the commitment to universals. Two theories may agree that numerals ought to be treated like predicates but disagree on predication. Interpreting numerals as predicates does not by itself impose any particular theory of prediction or of universals, for theories which differ on ontology can give similar semantic interpretations to number words. Frege's attacks on Mill exemplify this: while they were worlds apart metaphysically, each, in his own way, treated cardinal numbers as predicable of the bearers of number. Therefore, although I am viewing arithmetical truths and laws as involving natural kinds, the structural analogy between arithmetic and the natural sciences does not depend on the existence of natural kinds. We can press the one case on the other, and if there are no natural kinds, then the ontology of natural science will not involve natural kinds any more than the ontology of arithmetic. Thus natural kinds, and universals in general, would not be essential to the view proposed if a more nominalistic semantics for laws of nature were available. We might therefore think of the argument as showing the weaker, conditional claim that *if* universals exist, then the natural numbers are universals.

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[13] The notion of 'explanatory strategy' is due to Kitcher [1982].

This deflationary route will only work if a nominalistic semantics can be found. And this would, in fact, be much closer to the view espoused by Mill the empiricist and nominalist. Mill's theory of laws was of a kind with his empiricism, for he was a regularity theorist about laws: a statement of law merely expresses a universal generalization, and a law is nothing but an observed regularity. But if Mill's Regularity Theory of Law is to be adequate, there must be some account of how statements of law support counterfactuals; some account of modality must be provided, if only to dissolve it away. However he accounts for the modality, sentences [1] and [3] ought to receive similar treatment. Therefore, our position on arithmetic will be parsimonious about modality and universals just in case our position on the natural sciences is. If the two cases involve modality in exactly similar ways, so that a semantic theory will treat the corresponding generalizations in parallel fashion, the associated metaphysics will be neutral on the conclusion that truths of arithmetic are very like laws of nature. Whether we take on universals, or try to give a nominalistic semantics, it is a nice irony that the result will yield the homogeneous semantics which we put initially as a desideratum which motivated literalism in the first place.

5 BEYOND THE NATURAL NUMBERS

We have not dealt with quantified sentences of arithmetic. On the one hand there are general laws, like the commutative and associative laws for addition and multiplication. And there are existential sentences which, read literally, would commit us to numbers. Once we quantify over numbers, the picture changes if such sentences are to be regarded as playing an active role in applications. A theorem of either kind, a general law or an existential claim, does not usually occur directly as part of an application; it is more relevant in the realm of pure number theory. Nevertheless, it is instructive to see what such an application would look like, although it may seem far-fetched. Consider the existential claim

9. There exists a natural number between 2 and 4.

Suppose I want to line up brandy glasses with colored marbles so that for each natural number less than 10 I have one glass with that number of marbles in it, arranged in order from the left. At first I begin somewhat randomly, and I find I have a glass with 2 marbles in it, and to its immediate right, a glass with 4 marbles in it. I will then think to myself that something is missing: there ought to be a glass with 3 marbles in between the one with 2 and the one with 4. In the process, I appealed to my belief that there exists a natural number between 2 and 4. Recall our covering-law model of applications of arithmetic involving generalizations about aggregates. The theorem [9] is applicable to this situation because from it I can infer some generalization about aggregates of the appropriate cardinalities. It is not obvious, however, what the generalization will look like. One possibility is to bring in modality in a new way:

10. $(x)\,(y)\,(2x\ \&\ 4y \rightarrow$ It is possible for there to be an aggregate which has more elements than x and fewer elements than y.)

This generalization involves an essential use of the operator 'it is possible for . . .'. Any alternative would invariably involve some other problematic construction; most naturally, we would assert the existence of a *number*, whether or not a natural kind, which lies between 2 and 4 on the number ordering. But then, it seems, the generalization about aggregates is just as much about numbers; and arguably it is a restatement of [9].

This does not show, however, that the covering-law model of applicability is not correct and useful. There are two independent reasons for this. First, the guiding strategy in assimilating the case of arithmetic to science is to press the one case on the other, that is, to argue that in all relevant respects the two cases are analogous. Sometimes we can push one side of the analogy away from traditional beliefs and it will give a little, sometimes the other side will give. In the case of existential statements we found a new commitment either to modality or to number-properties. But this again has its analogue in science, for there, too, we find ourselves asserting the existence of higher level magnitudes and characteristics and kinds, and generalizing over them as well, especially in laws in the so-called 'special sciences'. For example, Elliott Sober argued that evolutionary theory may be irremediably committed to properties because explaining the existence of sexuality involves generalizations over a domain of properties. On Quine's criterion on ontological commitment, evolutionary theory is thereby committed to properties. An example of a law-like claim is: '[F]or any property P, if there is selection for greater offspring variance-in-P, and if sexual individuals are more likely to produce offspring with greater variance-in-P, then sexual individuals will be favored by selection.' Sober [1982] argues that the universal quantifier must be interpreted as ranging over properties and not over their extensions. This is just one of many examples we could use to argue that natural science is committed to the existence of kinds, properties or other kinds of universals.

Another consideration is that these sentences which involve quantification over numbers are going to be further removed from applications than the simple unquantified ones, and might be best treated instrumentalistically as theoretical effects of streamlining theory. Indeed, while the view sketched here has some of the virtues of plausibility and naturalness, the analogy becomes strained if we try to accommodate larger parts of arithmetic, not to mention more abstract areas of mathematics. Calculations that are more complex than addition among integral cardinals are less plausibly construed in terms of number-universals being attributed to aggregates in the world. In particular, this view cannot, I believe, adequately handle rational numbers. D. M. Armstrong claimed, but did not show, that his own version of a theory of numbers as universals can accommodate not only rational numbers, but reals as well (Forrest and Armstrong [1987]). I find it implausible to think that this sort of view could accommodate the rationals in a natural way. Some might object that we could never become acquainted with facts involving non-integral rationals. But this epistemological concern is less troublesome, I think, than a methodological concern: If we extend the picture of applications above to accommodate rational numbers, it would lose much of the elegance and plausibility that motivated it in the first place. The view here nicely accommodates the intuition that we predicate integral cardinal numbers to aggregates in counting and adding, but it was not intended as a foundation for mathematics.

There are some puzzling cases which, I think, ought to be accounted for by the view here. For example, we can use the non-integral rationals to count things, like one and a half apples on a plate. This seems clearly to be a case of counting, in which we quantify in exactly the same way that we use the integral cardinals; and it is puzzling because it surely uses the non-integral rationals essentially. But it indicates not that we ought to introduce universals corresponding to 'one-and-a-half' or to just 'one-half', but that we are extending our simple idea beyond its original bounds: this would be a place to introduce idealizations and approximations. On the other hand, it does not seem likely that measuring can be accounted for using these cardinality universals, except in so far as we are counting the units of measurement, like 3 feet or 2 quarts. For reasons of elegance and plausibility mentioned above, it would be a mistake to try to get all of arithmetic, much less analysis, accounted for as a species of natural science along the lines I've given here. Structuralism, the view that the objects of mathematics are structures, has much more promise for these matters. Part of the story of the applicability of mathematics involves these structures as they are instantiated in the world. If my picture here of applications of elementary cardinals is correct, a condition of adequacy for a structuralist view is not only that it be compatible with this picture, but that the two very different pictures fit together in a motivated non-*ad hoc* way.[14]

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Projection, Physical Intelligibility, Objectivity and Completeness: The Divergent Ideals of Bohr and Einstein

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ABSTRACT

It is shown how the development of physics has involved making explicit what were homocentric projections which had heretofore been implicit, indeed inexpressible in theory. This is shown to support a particular notion of the invariant as the real. On this basis the divergence in ideals of physical intelligibility between Bohr and Einstein is set out. This in turn leads to divergent, but explicit, conceptions of objectivity and completeness for physical theory.

1 *Introduction*
2 *Homocentrism and Projection*
3 *Ideals of Intelligibility*
4 *Objectivity*
5 *Completeness*
6 *Conclusion and Beginning*

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1 INTRODUCTION

Bohr or Einstein have this in common: neither were wedded to maintaining the world view of classical mechanics. Despite his appeal to classical concepts, Bohr was only driven to adopt the indispensability of these by his efforts to understand the conceptual significance of quantum theory. He constantly emphasised the importance of the non-classical character of quantum phenomena. (And he died claiming that we need still more shockingly strange experimental results in order to provide us with deeper insight into the present mysteries.) Einstein for his part was quite ruthless with classical space and time concepts in his pursuit of the special and general relativity theories and equally bold in his earlier papers on radiation/gas comparison, Brownian motion, the photoelectric effect and so on. Yet Bohr and Einstein chose utterly different ideals of physical intelligibility, objectivity and completeness.

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* I am indebted to Dr. G. McLelland, Professor F. Rohrlich and an anonymous referee of this journal for several improvements in the formulation of the paper.

It is my intention in this paper to offer a characterization of these divergent ideals. The analysis begins with an investigation of the roles of certain basic constructions in theoretical physics.

2 HOMOCENTRISM AND PROJECTION

The history of physics is punctuated by revolutions which mark the discovery by humans that what they had taken as central features of the world around them were in fact projections of their own condition. The passage from Ptolemaic to Copernican astronomy provides a simple case in point. Under the Ptolemaic system, our point of view as earth-bound observers was assumed unconsciously in this precise sense: it had no explicit representation in the theory. Yet there was one tell-tale sign that an unconscious homocentric point of view was being projected on the cosmos, namely the appearance of a mysterious non-causal harmony among the planetary orbits. Every planetary orbit, when represented in terms of epicycles, turned out to have exactly one motion in common and that coincided with the earth's seasonal period. From the Mediaeval point of view such celestial harmonic agreement might appropriately be the work of God, *i.e.* (more naturalistically) result from a common cause. Significantly, we know today that it would in fact be a non-local non-causal harmony, a fact which could be demonstrated by changing the motion of the earth and observing that all celestial bodies changed their corresponding component of motion simultaneously and in harmony. This peculiarity might strengthen the evidence of God's intervention did we not understand its origin in ourselves.

The transition from Aristotelian to Newtonian physics marks other, similar transitions in the explicit representation of our hitherto unconscious homocentric projections. Thus the Aristotelian notion of a well defined up/down direction is a homocentric projection of our gavitational and atmospheric circumstances (the one for rocks and other massive objects, the other for fire and other lighter-than-air phenomena). The Aristotelian law of motion, requiring an inner motor to drive it, is a projection of our own frictional circumstances which is given an explicit representation by a theory of frictional forces against the background of Newton's first law of motion. And so on. All Aristotelian motions were referred to the earth, but the earth itself was neither given nor denied a motion, it lay outside that category. The shift to Copernican astronomy, completed only in the development of Newton's mechanics, is a shift to a system of physics in which all objects fall under the category of motion, even those which may be held to be absolutely at rest (motion = 0). In this system we can recover our homocentric point of view by taking any true description and transforming it into a frame moving with the earth. In this way we may both learn to recognize explicitly that we have a point of view, and learn to transcend it so as to achieve a more objective assessment. In the larger framework, our homocentric point of view becomes but one perspective among many.

Indeed, from this point of view the use of spatio-temporal frameworks themselves is fundamental to the process of transcending our own egocentric and, ultimately, homocentric point of view. Developmental psychologists agree that notions of object constancy and dimensionality do not emerge in the young child's conceptual operations immediately at birth but are aquired over the first few years of post-natal experience. Significantly, they emerge in tandem with the developing notion of a separate self, initially centred in awareness of an individual body. We may understand this cognitive achievement as the representation in the brain of a 3-dimensional spatial framewortk which transcends any momentary egocentric point of view, and in which all of those points of view are represented as so many 2-dimensional perspective projections. In this way the young child is able to organize and unify experiences. At the same time that very conceptual apparatus provides the basic means for distinguishing one's own body from other things, and hence for representing the fact that one has an egocentric point of view. Prior to that time such a point of view was not explicitly representable, but it can now be represented as one point of view among others.[1]

Classical or Newtonian mechanics repeats this conception of the role of 3-dimensional space for the case of homocentric projection by the species as a whole. This is the import of the preceding discussion. Thus within both physics and cognition more generally we may think of our fundamental process for escaping unconscious egocentric/homocentric viewpoints as the construction of a higher dimensional space of possibilities within which each particular viewpoint is embedded as a possible lower-dimensional projection, a projection from that point of view. The structure of the embedding space determines how individual viewpoints are inter-related. The appropriate criterion for what is fundamentally real will then be what is invariant across all points of view. Thus cognitively we regard 3-dimensional physical objects as real and this is replicated in classical physics for the species as a whole.

It is this perspective which both allows us, and requires us, to treat the measurement process as one physical interaction among others, all equally embedded in the space of possible system–system relationships. The space-time framework as the embedding framework for diverse points of view will be disrupted if a representation of this sort cannot be provided for we shall not be able to represent ourselves, our own bodies, as measuring instruments. Whatever else it is, perception is itself a process in which information is transferred from external environment to a representation in the body of the preceiver. But the measurement process itself is essentially a transfer of information from a measured system to a representation in a measuring instrument. If measurement processes cannot be represented as physical interactions among others, then neither can perception. And then we shall have failed to construct a space in which our own bodies figure equally among other bodies. But this latter is the precondition for unifying our points of view in a single arena.

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[1] This was beautifully expressed many years ago by Bohm [1965], Appendix.

The transition from classical to relativistic mechanics parallels the great Copernican/Newtonian transition in important respects. We may understand the principle of relativity as beginning from the demonstration that our classical notion of simultaneity is in fact an unconscious projection of our own local space-time coordination, a coordination which has itself no explicit representation within Newtonian theory. This latter came about because human experience was for a long time effectively confined to low velocities. With hindsight one can provide a 4-dimensional representation of mechanics, including classical mechanics, in which the simultaneity co-ordination is explicit. The key feature to observe is that its structure becomes increasingly global as the velocity of light C is increased, and drops out altogether in the limit where C is very large relative to observed velocities. The 4-dimensional structure can then be uniquely factored into a 3-dimensional spatial structure and an orthogonal, hence independent, 1-dimensional time. But once the 4-dimensional relativistic formulation was achieved the co-ordination assumption suppressed by this independence could be explicitly represented for the first time.[2] We are then no longer unconsciously bound to its seeming characteristics.

There is a direct epistemic comparison between the construction of 3-dimensional space as sketched above, and Minkowski space-time. Relativity theory provides the means to construct the description of the universe from within any one of infinitely many inertial frames of reference (all frames in the case of the general theory), given a description within any one specified frame. We are then able to represent the sum total of all such relationships among frame-dependent descriptions by embedding the representative of each frame of reference in a single geometry, the Minkowski geometry. From this abstract point of view, any one frame of reference represents a particular frame-projection of that 4-dimensional geometry into a constrained structure, namely a local $3+1-$Euclidean geometry. In this way, Minkowski space-time is an abstract representation of the set of all possibilities, each specific actuality that is experienceable is confined to one particular frame of reference. What is physically real are those structures which are invariant across all frames of reference.

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[2] The inter-theory relations here are, however, quite subtle; see *e.g.* the discussion by Rohrlich [1988]. For an illuminating contemporary review of 4-dimensional formulations see Friedman [1983] and his references. The idea that the invariant is the real has a long history in epistemology, as long as that of philosophy itself, and in geometry itself it was given an explicit formulation in Klein's Erlangen program of 1872.

Minkowski space-time is intended to function as a representation which unifies all the individual points of view for the description of physical phenomena. We are then able to raise the following question: why is it, fundamentally, that the point of view defined by material object projects out of Minkowski space-time a $3+1$ – dimensional structure? What is it about the nature of matter that forces this kind of projective representation of its relationships with the cosmos? This question is not to be answered by simply appealing to the structure of Minkowski space-time, this would be simply to repeat the question's presupposition without addressing it. It will have to be understood, if at all, by understanding the fundamental constituents of which macroscopic objects are made and then understanding how those constituents relate to their most fundamental spatio-temporal features, the nexus as a whole generating just this kind of projection structure. In the orthodox presentation of relativity theory such questions have been avoided by simply assuming the existence of matter, undifferentiatedly the same at macroscopic and microscopic levels, and by assuming notions of signal and observation.

Let us appreciate the peculiar force and significance of the question within relativity theory. In the Minkowski geometry the paths of light signals are null geodesics in the standard formulation, $ds^2 = dx^2 - c^2(dt)^2 = 0$. Furthermore, dx^2, dt^2 individually approach 0 as one transforms to inertial frames approaching the velocity c. There is a natural physical interpretation: the points of emission and absorption are in causal contact for a photon; from a photon's point of view there is no physical separation between them (either in space, or in time, or in space-time). But this leads to a particular form of the preceding general question: Why is it that fundamental electromagnetic processes which, from the point of view of the photons involved, occur over no space in no time, nonetheless occur over a finite interval of space through a finite interval of time from the point of view of every material system? What is it about the nature of matter that, as it were, forces one to project what is evidently local (in contact) as non-local (as occupying a finite interval)?[3]

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[3] This particular form of the question, raised *e.g.* by Bohm [1962], has a complementary one from the photon's point of view: why is it that all other objects than photons generate infinite space and time intervals in the limit as frame velocity approaches c? Note that in all reference frames the light velocity remains invariant at c.

A referee of this journal posed this challenge: why not exploit the pseudo-metrical character of the standard formulation to postulate topological distinctness for points of 4-separation 0, and in this manner remove the specific puzzle posed in the text? But a move of this kind would evidently lack any physical motivation. The empirically confirmed causal structure is that induced on Minkowski space-time from the standard formulation and this defines a topology which corresponds to the usual topology. For discussion see *e.g.* Thirring [1978].

Whatever the complications, if any, the remarks of this note may be thought to introduce, the general question about the character of matter is wholly unaffected.

Though thoughts about the history of science as uncovering hitherto unconscious homocentric projections are old and widespread they were re-kindled in me by some comments in 1984 by my graduate student Mr Martin Leckey. Subsequently, the particular situation of

The two questions about relativity theory explicitly raised in the preceding two paragraphs I take to be the 'relativistic measurement problem'. This measurement problem is never really raised in conventional presentations of the subject, yet it clearly deserves an answer just as much as does the quantum measurement problem. Moreover, once posed the question seems as paradoxical and difficult as it does in quantum theory. Indeed, the only discussions which bear on the issue in the literature have to do with the representation of consciousness in Minkowski space-time diagrams. This line suggests that consciousness intrudes into the relativistic measurement process in something of the same way in which some hold that the conscious mind intrudes into the quantum measurement process. Such notions have bedevilled the understanding of Minkowski space-time as a representation of multi-viewpoint possibilities since its inception.

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relativity theory was made to stand out again by re-reading Bohm [1962]. In that paper Bohm draws a little diagram of four photon paths, as shown:

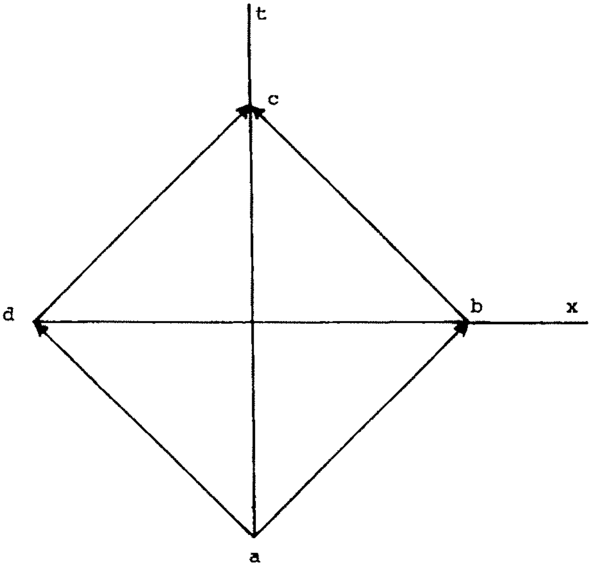

FIGURE 1

Bohm emphasizes the nullity of the path over which each of the photons moves, but asserts nonetheless that successive paths, ab+bc, ad+dc, create time intervals while 'opposite' signals, ab+ad, create spatial intervals. But all this is given without raising the question of how it can be possible for this diagram to be a finite size from our point of view, as it were, when each of its sides has null space, time, and space-time size. Nonetheless, it is here that Bohm first lays out the stimulating principle for fundamental physics research that the topological character of physical processes themselves should guide the development of physical theory. It is in terms of just such a principle that his own work, that by Finkelstein (*e.g.* [1979], [1988]) and many others (see *e.g.* Bastin [1971]) have followed.

Quantum mechanics can also be understood as offering a specification of physical systems from any one of a number of alternative frames of reference. These are roughly the 'measurement' frames. The set of all such possible relationships among 'measurement' points of view can be represented abstractly by embedding each of them in a single geometry, Hilbert space. Each 'measurement' reference frame then represents a particular frame-projection of this abstract space, namely the specification of an orthonormal basis for it, just as each relativistic reference frame determines a particular kind of coordinatization of the representation space. In both cases these coordinatizations are competing alternative coverings of the representation space. The actual measurement result itself then represents a further projection, a further confinement of the set of possibilities, just as it does in relativity theory. The structures which are invariant across reference frames are evidently the structure of possibilities specific to the quantum system concerned, operative nomic relations and all conserved quantities. It is these features which count as real, then, rather than any more specifically described or concrete objects.[4]

From this point of view the status of the 'measurement problem' is quite analogous to that in relativity theory. In neither case do we have any deep account of just why it is that the structure of possibilities should be represented in that particular unified manner nor why it is that material frames of reference project the particular geometrical coordinatizations of the unified space that they do. Quantum mechanics also provides no representation of the further projection which relates a reference (measurement) frame to the particular quantities observed. This is evidently unlike the case of relativity theory and extends the quantum measurement problem (*cf.* F2, footnote 18). Thus quantum mechanics represents a further retreat, in comparison to relativity theory, from the classical ideal of representing all possibilities in a unified structure within which each point of view and all its specific descriptions (measurement results) would appear as particular alternatives among many, understood by a single set of laws.

Why is it that relativity theory represents ourselves in one kind of unified structure and as materially determining one kind of frame-projection from it, while quantum theory represents ourselves in a quite different unified structure and as determining a quite different type of frame-projection? Precisely how does each theory relate a frame-projection to measurement outcomes and why are the two relations different? We are as yet not in a position to explore these issues in any penetrating way. We may note, however, that Minkowski space-time is a configuration space, a direct generalization of our basic cognitive representations, whereas Hilbert space is a more abstract Hamiltonian representation space. The classical version, phase space, assumes a certain formal independence between space-time and dynamical quantities (between position and momentum), which it retains consistently only because of the Weierstrauss limit properties (footnote 13 below). The quantum mechanical version retains an analogous construction but denies the independence. And we may note that whereas classical mechanics represents n-body systems in a single higher-dimensional phase space in which each component phase space is a sub-space, quantum mechanics represents them in a quite different space from that of their components, a tensor-product space. These differences could be extended.[5] We are not yet in a position to formulate these comparative questions in a uniform and penetrating way.

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[4] In both special relativity and quantum mechanics there are quantities which are always invariant across all reference frames. In relativity theory these are essentially the 4-dimensional geometrical features (intervals, angles, etc.) while in quantum mechanics they are the quantities obeying superselection rules (mass, charge, etc.). All other quantities may vary from frame to frame. For any particular material system, all conserved quantities will be frame invariant (though not their component values). In quantum mechanics frame varying quantities in general take on 'sharp' values in essentially only one frame (that in which they are 'measured') while in relativity theory frame varying quantities will take a velocity-independent value in only one frame (the rest frame).

To complete these reflections, let us entertain the notion that both relativistic and quantum measurement problems arise from unconscious homocentric projections, projections of some features of ourselves onto the cosmos as a whole. Projections which, unlike the earth's motion *vis-à-vis* Newtonian mechanics, have not yet been discovered and removed through explicit representation in a new theory. Certainly the quantum theory shares with the Ptolemaic theory certain deep-seated a-causal harmonies. Precisely the point of the coherency exhibited by systems of the Einstein–Podolsky–Rosen type, *e.g.*, is an a-causal spatially extended harmony of this sort. The extended structuring exhibited by Fermi–Dirac systems obeying the Pauli exclusion principle provides another instance (*cf.* F4, F5, footnote 18). If this understanding were correct, it would lead us to suppose that these puzzling features represented some deep-seated fact about the character of the human physical condition which needs to be made explicit and then embedded in a higher-order unifying representation. The relativistic representation, however, seems almost to suffer from the reverse problem. It has often been emphasised that the notion of a light signal is fundamental to the causal characterization of Minkowski space-time (specifying its conformal structure). Yet from the point of view of that signal itself there are no causal processes and no space or time intervals to characterize, rather they are replaced by contact. Here any supposed fundamental homocentric projection would be our projecting such a system of contacts as a finite space-time structure at all. In any event, it is evidently the case that both theories show structures which, while recognized, do not yet have a perspicuous representation in a unified possibility structure.

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[5] These issues are partially explored in Hooker [1973]a.

There are two fundamentally divergent responses to these problems. The first would be to look for some deeper characterization of matter in its relationship to both radiation and space-time which simultaneously specifies both the nature of the frame-projections determined by material systems and the nature of measurement processes as some interactions among many. This is the approach required by Einstein intelligibility. On the other hand, one may seek to understand the problem as primarily a conceptual one having to do with the limits on the coherent construction of unified possibility structures. The measurement problem would then simply be seen as yet a further expression of our inability to transcend the conceptual limitations of self-representation. This is the path dictated by Bohr intelligibility.

3 IDEALS OF INTELLIGIBILITY

Bohr could see no way to intuitively understand the world of quantum phenomena in the kind of way in which we evidently intuitively understand our human-sized world of classical phenomena. For him, therefore, all knowledge of the quantum world was knowledge by indirect description, employing abstract mathematical models for the purpose. Under these circumstances one requires a purely formal control over the interpolation of purely formal abstract mathematical constructions into intuitive physical descriptions. Therefore Bohr's ideal of intelligibility focused on the logical control of mathematical models *vis-à-vis* intuitive classical descriptions so as to ensure internal consistency and unity, empirical adequacy and minimal non-intuitive disruption of the applicability of intuitive (classical) concepts. Bohr referred to a conception meeting these conditions as a 'rational generalization' of classical physics.[6]

Einstein, by contrast, wanted to retain the essential ideal of classical physical intelligibility, namely that our physical theories should directly and correctly capture the intrinsic nature of the physical world. The world does not have to be classical in nature, but if it is to be intelligible it should be directly and correctly representable in our theories. Einstein was brief and somewhat ambiguous on these matters, but it seems clear when reading him overall that this correctly captures his position.[7] And it surely underlies Einstein's continual search until his death for a theory that would replace both quantum theory and relativity theory with some deeper underlying physical picture exhibiting this kind of intelligibility. He was prepared to see both of these very different extant theories as but approximations.[8]

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[6] See Hooker [1972] for discussion. There has been a recent resurgence of interest in Bohr, see *e.g.* Folse [1985], Honner [1987], Murdoch [1987]. Honner has almost nothing explicit to say about Bohr's notion of a rational generalization, while Folse and Murdoch give some account of it. (Murdoch refers to it as a kind of reduction relation, but this is not, I think, a good use of language.) Neither Folse nor Murdoch pursue its relation to fundamental constants in any depth. Both state its general connection to the limits $h \to 0$, $1/c \to 0$, but neither investigate, *e.g.*, its connection to e^2/hc (and after all Bohr did refer to quantum mechanics as a rational generalization of quantum electrodynamics), or attempt to understand Newtonian physics as a rational generalization of Aristotelian–Ptolemaic physics of this kind. See Hooker [1972], Section 13 and [1979] for some comments. See Friedman [1983], Rohrlich [1988] on $1/c \to 0$.

Einstein Intelligibility Ideal

ECI1: There is a clear distinction between elements of the theory representing physical reality and others (which are the 'idealizations') and each of the former elements uniquely and precisely characterizes the reality it represents.

ECI2: In a fundamental physical theory every element of reality and physical attribute in the domain of the theory has a corresponding counterpart in the theory. Descriptions of complex objects are given by complexes of descriptions of simple objects.

ECI3: A fundamental description of a physical system S during a time interval T is one for which every attribute possessed by S during T is uniquely specified. These descriptions can be given in a form invariant across reference frames. Other descriptions provide partial (frame dependent) information. Statistical theories represent the average behaviour and fluctuation characteristics of physical magnitudes over ensembles of otherwise completely describable systems and hence characterize a state of partial information only.

ECI4: Measurements are straightforward physical processes of interaction between a measuring instrument and a measured system as specified in some complete theory or theories. Physical systems exist and evolve independently of the presence of observers, *qua* observers.

DIAGRAM 1

[7] It is, *e.g.*, the thrust behind the EPR completeness criterion that 'every element of the physical reality must have a counterpart in the physical theory'. It also stands behind the reverse direction of the more controversial EPR requirement that 'if, without in any way disturbing the system we can predict with certainty (that is, with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity'. The former, if not the latter (see below) derives directly from Einstein.

[8] Fine [1986] paints Einstein as essentially an empiricist sceptic about metaphysics but a motivational realist, meaning that one must assume something like a realist stance in order to be motivated to explore contingent physical theory and experiment. Fine grants to Einstein's realism no more than this. This prepares the way to his own position, which is noncommittal either way. My own view is that Fine is mistaken about Einstein here, but in an illuminating way. I believe that Fine correctly captures an important feature of Einstein's thinking when he draws a distinction between the operational methodological level for a working scientist and some larger, more encompassing, framework. Operationally, Einstein was an avowed methodological opportunist; be an empiricist, even a severe operationalist, when it suits, be a realist, even a misty-eyed speculative metaphysician when it suits. But behind his operational opportunism there was always the recognition of an independent and even elegant, and intransigent precisely because elegant, reality. Such attitudes, indeed with even more nested levels of scepticism and commitment, are very much compatible with a naturalist realism (*e.g.* that advocated in Hooker [1987]).

together with an additional principle:

ECI5: The physical reality is exhaustively characterizable within the classical conceptual scheme of physical objects and attributes at definite spatio-temporal locations and each temporal sequence of states of a physical system S is such that each member of the sequence is uniquely causally or functionally generable from the immediately preceding members and a characterization of the physical environment of S.

In Hooker [1973] I tried to capture the classical ideal of intelligibility in nine principles. Modified and truncated a little, they can be summarized as those in Diagram 1.

As noted, we capture something like Einstein's ideal of intelligibility in the first four of these requirements, dropping only the fifth which is too dogmatically wedded to the classical idea.[9]

In like fashion we may define Bohr's position in terms of an ideal of intelligibility:

Bohr Intelligibility Ideal

BCI1: Every descriptive concept C employed by a physical theory has a set of well-defined and epistemically accessible conditions C_A under which it is unambiguously applicable.

BCI2: The set of descriptive concepts C employed by a complete physical theory between them exhaust the epistemically accessible features of the phenomena in the domain of the theory.

BCI3: There is a well-defined, unified and essentially unique formal symbolic structure to each complete physical theory which structures and co-ordinates its descriptions of phenomena such that each description is well-defined, the theory is formally complete and the set of descriptions between them exhaust the phenomena (use all applicable descriptive concepts).

BCI4: The fundamental concepts are those whose conditions for unambiguous application realize a well-defined boundary between observed and observing systems.

DIAGRAM 2

also together with an added fifth principle:

BC15: The concepts of classical physics are the fundamental descriptive concepts for science.

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[9] Petersen [1968] raises the intriguing and important question whether one should add to such requirements (and for Bohr also) that the theory be categorical, *i.e.* determine its own limits of valid application, but I set the issue aside here.

Though Bohr held strongly to BCI5, he would in principle have been prepared to give it up had circumstances demanded. (The relevant circumstances would need to show that BCI4 held for some non-classical concepts and not for some classical ones.) Despite popular academic opinion, it is best to leave BCI5 outside of Bohr's core notion of intelligibility (see footnote 6).

Bohr intelligibility departs substantially from that of Einstein intelligibility. Nonetheless, both Bohr and Einstein were concerned to eliminate ambiguity and vagueness. However, they went about it in divergent ways: Bohr by controlling the conditions of description, Einstein by focusing on referential isomorphism.

It may be possible to exhibit quantum theory so as to satisfy Bohr intelligibility.[10] Bohr connected the occurrence of the finite quantum of action precisely with central physical features of quantum mechanics which require to be elucidated, namely the indissoluble wholeness of composite quantum systems and the physical nature of measurement. And he connected all three of these in a very intricate way with an account of human conceptual capacities so as to form a tightly coherent story of the development of physics. Even the most radical alternative approaches do not succeed in going beyond these features and often prove less methodologically or conceptually satisfactory.[11] Should we then conclude that Bohr has said all there is to be said about the issue?

Unhappily, insightful though it is, there are many difficulties with Bohr's position (*cf.* footnote 10). The major ones are perhaps these two: First, it does

[10] The 'may' is there to signal residual uncertainty. Bohr went to great lengths to show how a presentation of quantum phenomena (as he called them) could consistently be offered utilizing his principles. Despite this there are macroscopic yet strongly non-classical phenomena like superfluidity and the Aharanov–Bohm effect which do not obviously fit the Bohrian mould. There is also the surprising and subtle wielding of relativistic principles which is required to rescue a Bohrian presentation of even non-relativistic quantum examples from difficulty (see Einstein's photon-in-a-box gedanken experiment). There is further the uncertainty of knowing how to extend the Bohrian account to include electromagnetic phenomena generally (*cf.* Hooker [1972], Section 13 on electric charge and Bohrian rational generalization). And there are other difficulties, surveyed in Hooker [1972], Section 14. All of which should give a Bohrian pause. Finally there is the explanatory failure of the Bohrian account (see below in text), but perhaps that is deliberate. (The new breed of Bohr books—see footnote 6—have relatively little to say about any of these problems, yet they are philosophically more astute than the earlier discussions of physicists; the acquisition of conceptual sophistication is evidently matched by a lowering of intuitive feeling for the physics.)

[11] Proposals like those of Margenau [1950] to simply assert an open-ended metaphysics of possibility, designed to do no more then make it so that quantum mechanical results emerge, are many—Wigner's use of consciousness to effect 'state reductions' is another example (Wigner [1963])—and without further precise specification amount simply to objectionable ad hoc-ing (*cf.* Hooker [1973]). Any further probing of these assumptions would require the actual physical (or anyway natural) processes to be specified whereby one could understand in detail how the peculiar quantum mechanical results emerged. The quantum logic approach has not yet succeeded even on its own terms. (This and related approaches to understanding quantum correlations are negatively evaluated in Hooker [1989].) These considerations apply also to epistemic approaches—see Hooker [1973].

not lead to any detailed physical account of what is happening in quantum processes, but rather places an absolute barrier to the conceptualization of such accounts at a level which is beyond, and even prior to, the actual doing of empirical science. In this way it generates a certain kind of unrevisable dogmatism. And for precisely this reason it fails to provide any deep guidance as to how to incorporate new features into physical theory, *e.g.* the discovery of further quantum 'particles', the reconciliation of the physics of gravity, electromagnetism and the strong and weak nuclear forces and so on. Second, as I noted long since (Hooker [1972]), the mere appeal to the exchange of quanta cannot in itself explain even why the measurement of one observable should randomize the outcomes for observables corresponding to non-commuting operators, let alone explain the particular structure of quantum mechanical conditional probabilities for the measurement values of other observables (*cf.* F7, footnote 18). Bohr describes quantum theory as a 'rational generalization' of classical physics. This it may be, but he is unable to explain why it is this particular rational generalization which applies in our world and not some other (see Hooker [1972], Section 14). This then leaves no grounds for a smug Bohrean dogmatism, but rather suggests that there is some deeper physical, and perhaps methodological and conceptual, insight which as yet eludes our grasp. Let us turn then to Einstein intelligibility.

It is possible to structurally refine the requirements of Einstein's ideal of intelligibility, in a certain direction, using the logico-algebraic conception of physical theory. The essence of the quantum logical idea is to extract an algebraic structure from the mathematical foundations of quantum mechanics and to understand the idempotents of this structure as corresponding to the propositions of a system of logic, in just the same way that the idempotents of a classical algebra correspond to the propositions of classical logic. In the classical case the propositions are of the sort 'The position of the system is between x_1 and x_2'; they may be understood as corresponding to subsets of phase space and statistical descriptions are given by suitable probability measures over them. For quantum mechanics the propositions are similar and the relevant algebraic structure of idempotents is that of the lattice of projections of a Hilbert space. One is then able to recast quantum mechanical descriptions of physical systems as propositions within the corresponding logical structure.

It is now well-known how classical particle mechanics and statistical mechanics are structured in relation to this ideal.[12] Such a representation provides an illuminating and sharp characterization of the structure of those theories. Despite this, the ideal of classical intelligibility has not in fact been achieved; there is no satisfactory corresponding representation of classical field theory, there is no satisfactory intrinsic characterization of atomic particles available and there are incipient incompatibilities among physical magnitudes which are suppressed rather than explicated.[13] Nonetheless, classical particle physics is the closest we have come to the classical ideal of intelligibility.

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[12] See *e.g.* Bub [1974] and Hooker [1973], [1975/79] and references.

From this perspective, the corresponding task for quantum mechanics can be summarized as the 'mirror image' of the classical analytical programme. This programme has not been completed structurally in many of its fundamentals (particularly Q set theory), though its more superficial layers come out in remarkable parallel to the classical particle physics paradigm.[14] This has given hope to the pursuit of an essentially particle metaphysics for quantum theory.[15]

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[13] With respect to classical particles, it is impossible to specify a coherent notion of contact in continuous space-time if classical particles are to be understood as characterized by boundaries which are always coincident with closed sets of the points of that space-time. Similarly, it is impossible to specify such particles as spatially extended yet fundamental objects (A-toms) without requiring interaction among their parts to proceed at infinite velocity and to obey an infinitely sharp cut-off at the boundaries, and hence to be essentially physically unanalysable and unrepresentable. Finally, classical momentum is specified in terms of a time derivative of position and hence, according to the Weierstrauss theory, logically is specified in terms of a limiting sequence of position differences. This is in contrast to position itself which is always specified at a point. It is only the Weierstrauss limiting process itself, made possible by the continuity conditions imposed on the configuration or phase space manifolds, which reduces the incipient momentum-position incompatibility found here to negligible proportions. But it does not alter its logical character. These considerations may be extended to energy-time relations though evidently not, interestingly, to classical spin component relations.

A self-consistent solution to the problems surrounding the well-definedness of classical particle physics evidently requires the introduction of a discrete or quantized geometry. And observe that one cannot introduce a discrete space together with a continuous time or vice versa without facing an indeterminacy argument: during transitions between the discrete elements the state of the system with respect to the continuous dimension must be undefined—see Hooker [1971] and F6, footnote 18. Whatever other difficulties there may be in this project (*cf. e.g.* Gudder [1968]), there is a fundamental conceptual difficulty to face: In a discrete space-time geometry it is impossible to uniquely identify particles by their space-time trajectories, since these cannot be uniquely identified whenever they are 'adjacent' and hence concern interacting particles.

With respect to classical fields, classical field theory has no structure corresponding to the phase space of classical particle mechanics and can have no such structure owing to the interrelations imposed by continuity constraints on classical fields and the inherently global character of the solutions to the partial differential equations which characterise classical fields (*cf.* the discussion in Hooker [1973]). In addition, there is no satisfactory statistical mechanics of classical fields. Each field occupies all space, so conventional ensembles are ruled out, and the mathematics of N even weakly interacting fields is essentially unknown. Finally, there has in addition been a problem with defining consistently and/or solvably self-interacting fields.

[14] The issue of a quantum set theory and other structural developments relevant to carrying out this task has been discussed in some detail in Holdsworth and Hooker [1983], where difficulties for its completion are also reviewed.

[15] Thus we have had a rash of recent writing offering a discussion of quantum metaphysics, almost all of it assuming a particle approach—see *e.g.* Forrest [1988], Maxwell [1988]. One is driven to 'smearons', 'propensitons' and other attempts to generalize particulate individuality in an effort to accommodate quantum structure. All the while the field or plenum structure, so radically different, is ignored, and the structural basis for particulate interpretations in quantum theory may be relatively superficial—see below in the text. There is much that is

However, the reasons for failure to complete this analytic programme may not be incidental. There are deep reasons to suppose that these structures are embedded in (what is known of) the structure corresponding to a classical field theory. But classical field theory has deep structural divergences from particle theory. Hence the structural origins of the traditional wave-particle duality.[16] Though I shall not pursue these arguments here, they give some ground for supposing that the project of completing a wholly 'particle' structure for quantum mechanics may not be achievable.[17]

It is quite unclear then how quantum theory can be made to satisfy Einstein intelligibility, but there is certainly no requirement that it can be made so only by re-introducing a classical mechanical base, as in the usual interpretation of the 'hidden variables' programme. Einstein showed little interest in such a programme and properly so (*cf.* Bell [1987], Fine [1986]). On the other hand, the more radical, if very different, researches of Bohm [1981] and Finkelstein [1988] *e.g.*, might both satisfy an ideal Einstein intelligibility. Unhappily, none of these research programmes has as yet come even close to furnishing a viable replacement for quantum theory.

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valuable in this revival of quantum metaphysics and represents a sharpening of earlier ideas, but again conceptual sophistication seems often to have been purchased at the expense of a balanced physical insight.

[16] For discussion of these differences and their relevance to the claims about the hybrid character of quantum mechanics see Hooker [1973]. The depth of the contrast between particle and field metaphysics is conveniently summarized as follows:

METAPHYSICAL SCHEMES FOR MODERN SCIENCE

	Atoms/Void	Plenum (Field)
Fundamental individuals	Atoms (many)	Plenum (one)
Properties	Reduced to natural kinds	No reduction, no natural kinds
Identity	Trajectory	Qualitative configuration
Change	Motion	Qualitative transformation
Dynamics	Cause (force)	Succession (harmony)
Continuity/∞	Discontinuous, finite (except space-time)	Continuous, ∞
Superposition	No	Yes
Number/Probability	Directly represented	Via integration and approximate abstraction

[17] Note that the logico-algebraic analytic programme, as described in the text, is also evidently inappropriate to capture relativistic theory. It assumes that time may be represented as an automorphism on the logic, and hence represented independently of the characterisation of the states of physical systems, but this independence fails in relativistic theory. Since the relationships between quantum mechanics and relativity theory are themselves intimate, if puzzling, there are further grounds for caution over the pursuit of this form of logico-algebraic analysis.

These cautionary remarks, allied to the deep metaphysical differences between field and particle, should give rise to caution in respect of those who argue easily to the conclusion that quantum field theory shows, by its existence and conventional textbook reading, that field and particle are equivalent representations. But this is a further story.

We are now in effect returned to those puzzling days of the 1930s when it was unclear how to proceed further with quantum metaphysics. We are, or should be, better off in two respects; we are in possession of a more comprehensive conception of the peculiarities of quantum theory, and we have a more penetrating understanding of the mathematical structures involved.[18] In any event, now that we have in hand the fundamental contrast in ideals of intelligibility, we are in a position to clarify also the equally conflicting notions of objectivity and completeness employed by Bohr and Einstein. Since these are the three fundamental concepts of philosophical appraisal for physical theories, we shall at least have a clarified framework within which to pursue analysis.

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[18] In fact appreciation of peculiarly quantum features is currently pretty fragmented, and often dominated by only one or two features, *e.g.* by Bell's theorem. The most salient features are these:

Problematic Features of Quantum Theory (QM)

F1: *Schrodinger Cat Paradox:* QM affords no clear-cut analysis of the stability and separability of macroscopic properties *vis-à-vis* the micro properties of which the macro constituents are composed. There is evidently no principled way, having a foundation within QM dynamics, to draw a macro/micro boundary.

F2: *Wave packet reduction:* The transition from QM state to determinate physical attribute, the so-called measurement process, has no representation within QM dynamics (unlike classical mechanics) and evidently has no representation within any other dynamics either.

F3: *Wave particle duality:* QM systems exhibit a peculiar coherency (arising from superposition) that undermines any attempt to follow the identity of constituents through such superpositions, despite particle number operators and other conserved quantities continuing to be well defined. Conversely, experimental phenomena show coherency characteristics yet determine only discrete quantized events. (Such coherencies may evidently appear also at the macroscopic level, *e.g.* superfluidity/conductivity.)

F4: *EPR Paradox:* QM systems exhibit a peculiar superposition coherency which issues in a certain kind of wholeness or non-locality which blocks construction of all traditional 'hidden variables' theories. Equivalently, n-body systems may display symmetries absent from their n-1-body sub-systems (including 'particle' components, *e.g.* the two-body EPR spin system displays rotational symmetry which the one-particle component states lack).

F5: *Condensation/Exclusion:* QM coherency together with the continuing well definedness of particle number operators and other conserved quantitites, and relativistic considerations, leads to the bifurcation between Fermi–Dirac (F–D) and Bose–Einstein (B–E) statistics, which has no classical analog. (The relation of these statistics to Maxwell–Boltzmann (M–B) statistics is itself part of the F_1 problem.) In particular the Pauli exclusion principle which characterizes the structuring of systems subject to F–D statistics is evidently not susceptible to a local causal interpretation.

F6: *Indeterminacy:* The existence of quantized transitions in the determinate values of physical attributes which take a finite time interval evidently logically requires that these attributes be indeterminate throughout the transition interval.

F7: *Probability Coherence:* From a determinate value for a single observable the transition probabilities for all other determinant values for all other observables uniquely follows, a feature which has no classical analogue. Moreover, QM states fundamentally determine only transition probabilities for defined observables, yet no determinate classical representation of these probabilities can be given.

F8: *Complementarity:* The particular relations among conjugate quantities in quantum mechanics generates the constraint on system descriptions known as the principle of complementarity. No satisfying metaphysics has been able to be provided for this principle.

F9: *Q-Properties:* The deeply non-classical character of QM 'spin' (eigen values bounded above, components do not commute), together with similar additional non-classical QM properties (strangeness, charm, etc.) have no satisfying metaphysical representation.

These features of quantum mechanics are discussed widely in the literature but it is unhappily rare to see them drawn together so as to bring into focus the multi-faceted character of the quantum mystery. Here, *e.g.*, reference to Bell's theorem is contained only in the third of these features and it is quite unclear how it could be used to illuminate any of the others.

4 OBJECTIVITY

To this divergence in ideals of intelligibility belongs also a divergence in the fundamental conception of scientific objectivity. For *Bohr-objectivity* cannot consist in removing the knowing subject from the representation of reality—precisely to the contrary. The drive for Bohrian intelligibility requires achieving the widest unified ('harmonious') rational generalization available. And by its nature, the very width of applicability of this generalization combined with its empirical adequacy reveals the most complete pattern of specification of physical phenomena accessible to knowers of our kind of world. And just this is what it is to be Bohr-objective, to achieve simultaneously both an empirically adequate, exhaustive and symbolically unified description of the phenomena we can produce and an accurate portrayal of the conditions under which such phenomena are accessible to us. In coming to know nature we come to know also ourselves as knowers, not fundamentally by being modelled in the theory as *objects* (although this also partially happens), but by the way the very form of the rational generalization itself reflects our being as knowing *subjects*.[19] Bohr's was a radical philosophy of physics, as he well knew, precisely because it involved rejecting the conception of objectivity which lay behind the achievements of the Newtonian and Einsteinian revolutions.

In sharp contrast, increasing *Einstein-objectivity* requires the successive elimination of the knowing subject from the form of the theory and its replacement by a representation instead in terms of the structure of objects modelled in the theory. For the ideal of Einstein Intelligibility requires that we strive to understand the knower as but one system among many and the knowing (read: measuring) relation as but one interaction among many. Increasing Einstein intelligibility is not aimed at reflecting the knowing subject but at achieving a representation of nature, including ourselves, in terms of invariances that precisely do away with reference to any subject's point of view. The latter may then be constructed from it. To become Einstein-objective is to be able to put ourselves into the models as *objects* in such a way as to take ourselves out of the picture as *subjects*.

For Bohr objectivity and subjectivity are jointly achieved. For Einstein they are contraries. Both, however, attempt to understand the history of science as a history of increasingly adequate and objective representations of nature.

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[19] This, incidentally, is the true Kantianism of Bohr's position—*cf.* Hooker [1972], Section 12.

Einstein intelligibility enjoins us to understand the history of science as the successive transcendence of old conceptual schemes, each achieving increased representational accuracy over its predecessor with the concomitant removal of homocentric viewpoints as we go. Bohr intelligibility enjoins us to understand that history as the successive refinement and rational generalization of commonsense phyical.concepts and experiences, revealing ever subtler layers of the involvement of humans as knowers in the form which science takes. Both conceptions are worth pursuing and both have been pursued in part by their great progenitors.[20] It would be illuminating to see these programmes for the history of science carried through in detail and compared.

5 COMPLETENESS

We are now in a position, I believe, to provide a more perspicuous discussion of the notion of completeness for a physical theory than is found in the conventional literature. The problem of completeness is not usually discussed within classical theory since the notion was taken to be obvious. Bub [1974] has pointed out very clearly that it is far from obvious what might be understood as a relevant notion of completeness for quantum theory. And this despite the fact that the controversy surrounding the Einstein–Podolsky–Rosen argument and hidden variable theories quite clearly raised this issue. Bub noted that the notion of classical completeness which formed the backdrop to the quantum considerations was evidently a formal one, roughly that a theory was complete if it could be embedded in a classical phase-space representation equipped with a Hamiltonian dynamics such that all probability measures could be interpreted as measures of ignorance. From this conception Bub developed a formal notion of completeness, namely that a theory should have exactly those states which can be distinguished from one another by that theory's allowed probability measures. Classical mechanics satisfies this ideal since the collection of its probability measures is a convex structure whose basis is a set of measures concentrated on the points of phase space, which themselves represent exactly allowed physical states of a system. The Hamiltonian dynamics then specifies the time-evolution of the states and hence of the probability measures over them. But quantum mechanics also satisfies this criterion, since it can be shown that the set of probability measures is a convex structure whose elementary basis is comprised of the 1-dimensional projections in Hilbert space which are in turn the collection of all 'pure states' of quantum systems. And the Hamiltonian dynamics specifies a time evolution in Hilbert space which determines the evolution of probability measures over these 'states'.[21]

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[20] See *e.g.* Einstein [1954], [1959] and Bohr [1958], [1963].

[21] These scare quotes around *state* here have to do with reservations deriving from the difficulty of understanding quantum mechanics as a purely particle mechanics—see Section 3 above.

What Bub has distinguished is a particularly important notion of completeness which I shall call *formal completeness*. It really has to do with the interior formal structure of the theory itself, with its adequacy to distinguish within its resources a sufficiently rich structure to provide a coherent specification of probability at all. But it does not touch the material relations between a theory and the world. These were the relations at the heart of Einstein's conception of physical theory and chiefly in dispute between he and Bohr. I shall therefore introduce another conception, Einstein completeness. A theory is *Einstein complete* if and only if it is Einstein Intelligible. A particular implication is that a theory is Einstein complete only if it has a representation of measurement processes within it as some interactions among many, embedded in a unified possibility structure and understood through a single set of dynamical laws. Quantum mechanics is certainly not Einstein complete. Neither, I have suggested, is relativity theory as yet.

Note that Einstein completeness does not entail classical completeness; it is quite compatible with a variety of non-classical worlds, presumably including at least relativistic worlds. On the other hand, it seems quite reasonable that Einstein completeness should require formal completeness. For any Einstein complete theory that was not a formally complete theory must either have dynamically distinguished states which can never be distinguished by any statistical means or possess redundant probability measures whose formal coherence must remain dubious.

In a similar fashion we may define *Bohr completeness* as satisfying Bohr intelligibility. Like Einstein completeness, Bohr completeness does not require classical completeness but demands formal completeness. But unlike Einstein completeness, it does not demand an explicit representation of the measurement process as one among many interactions, to the contrary it explicitly exempts such a process from representation, regarding it instead as part of our representing relationship to reality. Though there are many notions of completeness in the literature, Bohr, Einstein and formal completeness, together with their particular applications in classical and (presumably) relativistic and quantum completeness, provide, I suggest, the fundamental notions with which to engage the philosophical issues.[22]

6 CONCLUSION AND BEGINNING

The great intellectual adventure of physics has been the pursuit of intelligibility, objectivity and completeness. But where will this pursuit lead? We cannot yet tell. The debate has deepened our conception of the issues over the years but fundamentally we are turned to the debate between Bohr and Einstein. Perhaps there will emerge from the exploration of relativistic quantum field theory, quantum gravity, quantum cosmology and the like quite new notions of intelligibility, objectivity and completeness, notions which will transcend the debate between Bohr and Einstein. (Certainly the present debates, even that over Bell's theorem, typically do not.)[23] Perhaps, with Descartes, this pursuit will reveal the innards of the world machine; perhaps, with Newton, we shall glimpse the face of the divine wholeness behind our homocentric reality.

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[22] For example, Fine [1986] distinguishes what he calls *Born completeness*, which is really classical completeness by another name; *bijective completeness*, which is really one component of Einstein completeness, and a vague notion which he calls *Schrodinger completeness* which, insofar as it has definite content, is evidently a sub-case of formal completeness.

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[23] An argument for this negative decision in the case of Bell's theorem is given in Hooker 1988. See also the review of recent writing on quantum field theory in Hooker 1990.

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Intersubjective Probability and Confirmation Theory*

DONALD GILLIES

ABSTRACT

This paper introduces what is called the intersubjective interpretation of the probability calculus. Intersubjective probabilities are related to subjective probabilities, and the paper begins with a particular formulation of the familiar Dutch Book argument. This argument is then extended, in Section 3, to social groups, and this enables the concept of intersubjective probability to be introduced in Section 4. It is then argued that the intersubjective interpretation is the appropriate one for the probabilities which appear in confirmation theory whether of a Bayesian or a Popperian variety. The final section of the paper states and tries to answer an objection due to Putnam.

1 *Introduction*
2 *Subjective Probability and the Dutch Book Argument*
3 *Extension of the Dutch Book Argument to Social Groups*
4 *The Concept of Intersubjective Probability*
5 *Intersubjective Probability as Intermediate between Degree of Radional Belief (the Early Keynes) and Degree of Belief (Ramsey)*
6 *The Probabilities Needed for Confirmation Theory*
7 *An Objection: The Game Against Nature*

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1 INTRODUCTION

Subjective probabilities are often introduced by using the Dutch Book argument. The aim of this paper is to extend the Dutch Book argument to social groups, and thereby to introduce the concept of what I shall call *intersubjective probability*. It will then be argued that this notion of probability is

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[*] The first version of this paper was read by Professors M. B. Hesse and D. V. Lindley whose helpful comments are acknowledged in more detail below. The writing of a companion paper with my wife on 'Intersubjective Probability and Economics' (Gillies and Gillies [1991]) produced a radical revision of Section 4, which incorporates an addition suggested by Dr Fred Benenson. Professor Mary Douglas drew my attention to the relevance of her grid/group theory and its use by Bloor in his [1978] (*cf.* Section 6). I am also grateful for the many helpful comments I received when I read the paper in the Department of History and Philosophy of Science at Cambridge in January 1990.

the appropriate one for use in confirmation theory. Before proceeding to intersubjective probability, however, it will be helpful in the next section to give a brief sketch of the standard treatment of subjective probability and the Dutch Book argument. This will be a useful background for the developments which follow.

2 SUBJECTIVE PROBABILITY AND THE DUTCH BOOK ARGUMENT

The subjective or personalist view of probability was devised independently by Ramsey in England and De Finetti in Italy. Ramsey [1926] and De Finetti [1937] are two classic expositions of the theory. The basic idea is that probability is the measure of the degree of belief of a particular individual. Suppose for example that Mr A, Ms B and Master C all possess the same background knowledge and are considering some future event E—say that it will rain in London the next day. Mr A, Ms B and Master C may well each have his or her own personal or subjective degree of belief in rain in London the next day, and these three degrees of belief may be different.

But how are we to measure the degree of belief of Ms B say in E? Ramsey observed in this connection:

> The old-established way of measuring a person's belief is to propose a bet, and see what are the lowest odds he will accept. This method I regard as fundamentally sound; . . . (Ramsey [1926], p. 73).

De Finetti in his [1937] also adopted this betting approach although later, as we shall see, he came to prefer another method. With great ingenuity, Ramsey and De Finetti devised a chain of reasoning by which, starting from the general idea of measuring beliefs by bets, they were able to derive the axioms of probability. This has come to be known as the Dutch Book argument. Our own exposition will largely follow De Finetti [1937]. In particular, we will regard the bets as being made in money as De Finetti does in his [1937]. Ramsey in his [1926] takes the bets as being in units of utility, and develops a theory of utility for this purpose. I am sceptical about the possibility of measuring utility, and so prefer the money approach.

The first step is to specify the betting situation rather more precisely. This is done as follows. We suppose that we have two individuals A and B, and that A is trying to measure B's degree of belief in some event or events. We can imagine A as an experimental psychologist and B as his or her subject for investigation. A carries out the measurement by forcing B to bet under the following conditions:

Specification of Betting Situation

Suppose A is betting against B on the outcome of some event or events. B has to choose a betting quotient q for each event; while A chooses the stake S for each bet, where S may be $+ve$ or $-ve$ but $|S|$ is small. B gives A qS in exchange for S if the event in question actually occurs.

Observations

(i) The fact that S can be $+ve$ or $-ve$ is merely a device to ensure that A can choose whether the bet is for or against the events occurring.
(ii) When we say that $|S|$ is small, this must be understood in relation to B's wealth. The idea is to choose a level sufficiently high to force B to think about the matter, but not so high that B runs the risk of being ruined by the bet. After all, A is forcing the unfortunate B to bet as part of a psychological experiment.

The claim is that if B in the above betting situation chooses betting quotient q on event E, then q will measure B's actual degree of belief that E will occur. This claim seems to me very plausible.

From this specification of the betting situation, we can obtain the following form of what has come to be known as the Ramsey–De Finetti Theorem.

Ramsey–De Finetti Theorem: A will be able to choose the stakes so that he or she gains money from B whatever happens, *unless* B chooses his or her betting quotients to satisfy the standard axioms of probability.

Proof: See De Finetti ([1937], Chapter 1, pp. 100–10).

Observations:

(i) We here take the standard axioms of probability as involving finite rather than countable additivity. There has been some interesting discussion of whether the Ramsey–De Finetti Theorem can be extended to include countable additivity. However, we will not go into this issue here as it is not relevant to the main theme of the paper.
(ii) If A can choose the stakes so that he or she gains money from B whatever happens, then A is said to have made a Dutch Book against B. This explains the phrase: 'the Dutch Book argument'.

As Ramsey puts it:

> These are the laws of probability, . . . If anyone's mental condition violated these laws, . . . He could have a book made against him by a cunning bettor and would then stand to lose in any event (Ramsey [1926], p. 80).

Although De Finetti gives an admirable exposition of the Dutch Book argument in his [1937], he became disatisfied later in life with this foundation for subjective probability. Indeed in one of his last published papers he wrote:

> . . . betting, strictly speaking, does not pertain to probability but to the Theory of Games . . .

> It is because of this that I invented and applied in experiments (probabilistic forecasts) the 'proper scoring rules':... (De Finetti [1981], p. 55).

De Finetti's later approach is based more on decision theory than on betting. Despite De Finetti's doubts, my own view is that the Dutch Book argument provides an admirably clear and satisfactory foundation for subjective probability.

The converse of the theorem also holds, namely:

Converse of Ramsey–De Finetti Theorem: If B chooses his or her betting quotients to satisfy the standard axioms of probability, then it will not be possible for A to make a Dutch Book against B.

So far we have merely expounded some standard results concerning subjective probability, but we are now in a position to begin making the transition from subjective to intersubjective probability.

3 EXTENSION OF THE DUTCH BOOK ARGUMENT TO SOCIAL GROUPS

We proceed to the extension to social groups. Suppose A is betting against a social group $\mathscr{B}=(B_1, \ldots, B_n)$ of individuals. For simplicity take $n=2$ initially.

Theorem 1: Suppose A is betting against $\mathscr{B}=(B_1, B_2)$ on event E. Suppose B_1 chooses betting quotient q_1 and B_2 q_2. A will be able to choose stakes so that he or she gains money from $\mathscr{B}$ whatever happens *unless* $q_1=q_2$.

Proof: Suppose without loss of generality that $q_1>q_2$. Suppose A chooses $S>0$ on his or her bet with B_1, and $-S$ on his or her bet with B_2. Then if E occurs, A's gain G_1 is given by:

$$G_1=q_1S-S-q_2S+S=(q_1-q_2)S$$

If E does not occur, A's gain G_2 is given by:

$$G_2=q_1S-q_2S=(q_1-q_2)S$$

It is clear that $G_1>0$ and $G_2>0$, unless $q_1=q_2$.

Acknowledgement: Theorem 1 was suggested to me by Ryder's [1981]. In this important paper Ryder gives a result ([1981], p. 165) which is a special case of Theorem 1. Ryder uses this result to draw philosophical conclusions which are different from my own. I shall use Theorems 1, 2 and 3 to introduce the concept of intersubjective probability. However, I see intersubjective probabilities as additional to, rather than in contradiction with, subjective probabilities. Ryder, on the other hand, regards his result as showing that the whole subjective approach to probability based on the Dutch book argument is not viable. I will state and discuss Ryder's argument on this point in the next section.

The generalization from 2 to n is perfectly straightforward.

Theorem 2: Suppose A is betting against $\mathcal{B} = (B_1, B_2, \ldots, B_n)$ on event E. Suppose B_i chooses betting quotient q_i. A will be able to choose stakes so that he or she gains money from $\mathcal{B}$ whatever happens, *unless* $q_1 = q_2 = \ldots = q_n = q$.

Proof[1]: Suppose the q_i are not all equal. Then there must exist q_j and q_k such that $q_j > q_k$. Suppose A chooses $S > 0$ on his or her bet with B_j, $-S$ on his or her bet with B_k, and $S = 0$ on his or her bets with B_i where $i \neq j$ and $i \neq k$. Then arguing as in the proof of theorem 1, we conclude that A gains money from $\mathcal{B}$ whatever happens. Thus A can gain money from $\mathcal{B}$ whatever happens, unless $q_1 = q_2 = \ldots = q_n = q$.

Theorem 3: Suppose A is betting against $\mathcal{B} = (B_1, B_2, \ldots, B_n)$ on events $E_1, \ldots, E_r$ where $r \geqslant 1$. Suppose B_i chooses betting quotient q_{ij} on event E_j. If (i) $q_{1j} = q_{2j} = \ldots = q_{nj} = q_j$ for $1 \leqslant j \leqslant r$, and (ii) the q_j satisfy the standard axioms of probability, then it will not be possible for A to make a Dutch Book against $\mathcal{B}$.

Proof: If condition (i) is satisfied, then the group can be considered as a single individual with betting quotient q_j on E_j for $1 \leqslant j \leqslant r$. The result then follows from the converse of Ramsey–De Finetti theorem using condition (ii).

4 THE CONCEPT OF INTERSUBJECTIVE PROBABILITY

Informally what theorems 1, 2 and 3 show is this. Let $\mathcal{B}$ be some social group. Then it is in the interest of $\mathcal{B}$ as a whole if its members agree, perhaps as a result of rational discussion, on a common betting quotient rather than each member of the group choosing his or her own betting quotient. If a group does in fact agree on a common betting quotient, we shall call that betting quotient the *intersubjective* or *consensus* probability of the social group. This type of probability can then be contrasted with the *subjective* or *personal* probability of a particular individual.

The question now arises: 'Under what conditions will a social group form an intersubjective probability?' The following conditions seem to be of crucial importance:

(i) (Common Interest) The members of the group must be linked by a common purpose; whether the common purpose leads to solidarity or rivalry within the group does not matter much; the important point is that the members have an interest in acting together and reaching consensus; love or fear would create, in this case, similar bonds. The common purpose might be financial, but need not be; for example, a group of soldiers might have the common purpose of taking an enemy position with the minimum injury and loss of life to the group.

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[1] This proof was suggested to me by Prof. D. V. Lindley (Private Correspondence).

(ii) (Flow of Information) There must be a flow of information between the members though it does not matter whether the communication is organized centrally or peripherally or whether it is direct (between any two members) or indirect (through the intervention of third parties).

Condition (i) (Common Interest) means that the size and composition of the group can change as individual members may decide to break away if and when they reckon that they can gain by 'going it alone'; equally, new members may join the group when they recognize a community of purpose with it. The common purpose must be strong enough to bond the members together in relation to particular events, though this need not rule out individual members planning to break away or to gain at the expense of others on different issues. Another related point is that the propositions whose intersubjective probabilities are sought must be connected with the common purpose. Consider, for example, the group of Italian expatriots living in London. This group might well form a consensus probability regarding the question of whether there will be new regulations within a few years making it possible for Italian nationals living in the UK to vote in local elections. However, it seems unreasonable that the group should form an intersubjective probability concerning the number of king penquins on Elephant Island in the South Pacific.

There may be a problem in satisfying conditions (i) and (ii) if the group is very large. However, consensus probabilities may still be possible in this case provided there is an agency or association or union to organize the group including the flow of information within it.

The concept of intersubjective probability has, so I believe, possible applications in a number of different areas. One of these is economics. What makes intersubjective probability an appealing concept for economic analysis is the conjunction of the following two plausible claims:

(i) the beliefs of economic agents in situations of uncertainty will affect the economy; and

(ii) a particular degree of belief will only have an effect if it is shared by a fairly extensive group of economic agents.

To pursue these claims further would, however, raise rather technical questions in economics, and I will therefore not do so here. The matter is dealt with in a companion paper on 'Intersubjective Probability and Economics' (Gillies and Gillies [1991]). In the present paper I will consider an application to a central area of philosophy of science: namely confirmation theory. As I will argue in more detail in Section 6, most versions of confirmation theory involve the consideration of probabilities at some stage. The question therefore arises: 'What interpretation should be given to these probabilities?' My answer of course is that they should be regarded as the intersubjective probabilities of the social group of scientists in the relevant field.

Although there do seem to be a number of possible applications for the concept of intersubjective probability, I am very far from claiming that it is the only valid interpretation of the probability calculus. Consider for example the probabilities which occur in the natural sciences, such as the probability that a particular radioactive atom will disintegrate in a given time period. I regard such probabilities as fully objective features of the natural (non-human) world, and as in no way dependent on the consensus of beliefs in human social groups. I am not therefore repudiating the concept of objective probability developed in my [1973].

Nor, on the other hand, am I suggesting that intersubjective probabilities should completely replace subjective probabilities. The use of the first concept does not exclude the use of the second, but rather demands the use of the second. If, for example, $p(E)$ is the intersubjective probability assigned to E by social group $\mathscr{B}=(B_1, B_2, \ldots, B_n)$, then each member B_i of $\mathscr{B}$ assigns the subjective probability $p(E)$ to E. Moreover there may well be sets of individuals who do not reach a consensus and who therefore have each a subjective probability without there being any intersubjective probability. Indeed, as will emerge in what follows, my own view is that both subjective and intersubjective probabilities are needed for the analysis of human belief. Thus there seem to me to be three concepts of probability: subjective, intersubjective, and objective.[2]

Having just defended the concept of subjective probability, it is now appropriate to consider Ryder's objection to subjectivism in his [1981] which was mentioned in the previous section. Ryder states his argument as follows:

> Subjectivists accept that different individuals have different degrees of belief, but not much thought has been given to applying the Dutch Book argument to the situation where there is more than one person.
>
> If we have two (or more) people with different degrees of belief in the same simple event E, a Dutch Book can be made against them. This is just as 'disastrous' and 'obviously unreasonable' as it is for an individual. It means that subjectivists never actually make the bets which are envisaged by the Dutch Book argument. If they did someone could come along and find two or more subjectivists with different degrees of belief and make system of bets which would

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[2] For those familiar with Popper's theory of 3 worlds, we can point out the following rough correspondences:

subjective relates to World 2
intersubjective relates to World 3
objective relates to World 1

cf. Popper ([1972], Chapters 3 and 4).

> result in a certain loss to the subjectivists considered as a group (Ryder [1981], p. 165).

This argument of Ryder's is both plausible and ingenious, but I think that it can, nonetheless, be answered. To do so let us consider our set of individuals $\mathscr{B} = (B_1, B_2, \ldots, B_n)$ and the experimental psychologist A. Suppose first that A makes a Dutch Book against B_1. It is very likely that B_1 will regard this as 'disastrous' and 'obviously unreasonable', since he or she will lose money whatever happens. Suppose, however, that A makes a Dutch Book against the set $\mathscr{B}$ as a whole, but *not* against B_1 in particular. Will B_1 regard this situation as 'disastrous' and 'obviously unreasonable'? The answer is that B_1 may do so, but he or she need not necessarily do so. To see this let us consider two different cases.

Case 1: $B_1, B_2, \ldots, B_n$ have formed an arrangement whereby any gains or losses they make individually in their various economic activities are pooled, and the total divided equally between the individual members of the group. In this case, if A makes a Dutch Book against the set as a whole, then each of its members (including B_1) will suffer. Thus B_1 has to regard this situation as 'disastrous' and 'obviously unreasonable'. Note that this example has been constructed so that our condition (i) (Common Interest) is satisfied. Given such an arrangement, it would obviously be desirable for the group to ensure that condition (ii) (Flow of Information) is also satisfied, so that the group can form a consensus through discussion and have an intersubjective probability, thus rendering it impossible for A to make a Dutch Book against $\mathscr{B}$.

Case 2: $B_2, \ldots, B_n$ are more or less randomly selected individuals whom B_1 neither knows nor cares about. In this case A is likely to be able to make a Dutch Book against the group $\mathscr{B}$ as a whole, but B_1 is unlikely to regard this as 'disastrous' and 'obviously unreasonable'. Provided no Dutch Book can be made against B_1 personally, why should he or she care about what happens to the other unknown members of the group?

The key point is that the extension of the Dutch Book argument to groups is only significant for groups which have a common interest. The argument shows that such groups ought to establish communication and flow of information within the group so that they can form through discussion a consensus or intersubjective probability. Only in that way can the group as a whole protect itself against cunning opponents. It is a matter of common experience that there do exist such groups with a common interest and that they do often reach consensus in their beliefs.

If, however, we are dealing with a group which lacks a common interest, then the extension of the Dutch Book argument to groups has no validity, for each individual will then be indifferent to what happens to the other members of the group. In this case each individual will form his or her own subjective probability without any regard for the beliefs of the others.

5 INTERSUBJECTIVE PROBABILITY AS INTERMEDIATE BETWEEN DEGREE OF RATIONAL BELIEF (THE EARLY KEYNES) AND DEGREE OF BELIEF (RAMSEY)

One helpful way of regarding the intersubjective interpretation of probability is to see it as intermediate between the logical interpretation and the subjective interpretation. This matter can be clarified by a brief consideration of the debates concerning logical and subjective probability in Cambridge in the first three decades of this century. W. E. Johnson gave lectures on the logical interpretation of probability in Cambridge, and this view was adopted by Keynes and Jeffreys who both attended his lectures. The logical interpretation was then sharply criticized by Ramsey in his [1926], and this criticism led Ramsey to introduce the subjective interpretation. As Ramsey attacks Keynes rather than Johnson or Jeffreys, let us next consider briefly Keynes' version of the logical interpretation of probability.

The fundamental idea of this approach is that probability is a logical relation between evidence and conclusion. As Keynes himself puts it:

> We are claiming, in fact, to cognise correctly a logical connection between one set of propositions which we call our evidence and which we suppose ourselves to know, and another set which we call our conclusions, and to which we attach more or less weight according to the grounds supplied by the first . . . It is not straining the use of words to speak of this as the relation of probability (Keynes [1921], pp. 5–6).

Keynes also characterizes probability as *degree of rational belief*:

> Let our premises consist of any set of propositions *h*, and our conclusion consist of any set of propositions *a*, then, if a knowledge of *h* justifies a rational belief in *a* of degree α, we say that there is a *probability-relation* of degree α between *a* and *h* (Keynes [1921], p. 64).

The first problem which arises for Keynes is how we get to know about these putative logical relations of probability. He answers that in some cases at least we have a kind of perception of the logical relation. 'We pass from a knowledge of the proposition *a* to a knowledge about the proposition *b* by perceiving a logical relation between them' (Keynes [1921], p. 13). This view is strongly attacked by Ramsey.

> But let us now return to a more fundamental criticism of Mr. Keynes' views, which is the obvious one that there really do not seem to be any such things as the probability relations he describes. He supposes that, at any rate in certain cases, they can be perceived; but speaking for myself I feel confident that this is not true. I do not perceive them, and if I am to be persuaded that they exist it must

> be by argument; moreover, I shrewdly suspect that others do not perceive them either, because they are able to come to so very little agreement as to which of them relates any two propositions (Ramsey [1926], pp. 65–6).

This is an interesting case of an argument which gains in strength from the nature of the person who proposes it. Had a less distinguished logician than Ramsey objected that he was unable to perceive any logical relations of probability, Keynes might have replied that this was merely a sign of logical incompetence, or logical blindness. Indeed, Keynes does say: 'Some men—indeed it is obviously the case—may have a greater power of logical intuition than others' (Keynes [1921], p. 18). However, it would not have been very plausible for Keynes to claim that Ramsey was lacking in the capacity for logical intuition or perception—nor did Keynes do so!

The second problem for Keynes concerns the justification of the axioms of probability. Once again Keynes has to appeal to some kind of logical intuition, and once again this is a dubious and unsatisfactory procedure. Ramsey, on the other hand, was able to give a precise derivation of the axioms of probability using the Dutch Book argument. A third difficulty for Keynes' logical interpretation of probability concerned what Keynes called the Principle of Indifference. Keynes needed this principle to obtain numerical values for probabilities, and, as he himself very honestly pointed out, the principle gives rise to a number of paradoxes which are difficult, if not impossible, to resolve (see Keynes [1921], Ch. IV, pp. 41–64).

Ramsey summed up the situation as follows:

> We shall, I think, find that this view of the calculus of probability removes various difficulties that have hitherto been found perplexing. In the first place it gives us a clear justification for the axioms of the calculus, which on such a system as Mr. Keynes' is entirely wanting. For now it is easily seen that if partial beliefs are consistent they will obey these axioms, but it is utterly obscure why Mr. Keynes' mysterious logical relations should obey them. We should be so curiously ignorant of the instances of the relations, and so curiously knowledgeable about their general laws.
>
> Secondly, the Principle of Indifference can now be altogether dispensed with; . . . To be able to turn the Principle of Indifference out of formal logic is a great advantage; for it is fairly clearly impossible to lay down purely logical conditions for its validity, as is attempted by Mr. Keynes (Ramsey [1926], p. 85).

Ramsey's criticisms of the logical view of probability seem to me very convincing, and indeed I do not think that it is possible to give a satisfactory logical interpretation of the usual calculus of probability.[3]

This conclusion is reinforced by an empirical consideration of human belief patterns. If there was a valid logical interpretation of probability, and a single rational degree of belief in some conclusion *c* given evidence *e*, we would expect nearly all human beings to have this single rational degree of belief in *c* given *e*, since, after all, most human beings are rational. Such a broad consensus does indeed exist as regards deductive logic. Nearly all human beings who have acquired the technical background needed to understand the question will agree on whether a given chain of logical deductions is valid or invalid. Of course this consensus is not complete. There are indeed a few intuitionists and other believers in various forms of non-standard logic. However, the agreement in judgement is considerable even if not total.

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[3] Somewhat more detailed arguments for this conclusion are to be found in my ([1988a], pp. 183–7). Strictly speaking what I reject is a logical interpretation of probability of the traditional as opposed to 'topping-up' variety. For this distinction, see my ([1988a], pp. 194–5).

Far otherwise is the case of judging the degree of belief which evidence *e* warrants in conclusion *c*. Here different individuals may come to quite different conclusions even though they have the same background knowledge and expertise in the relevant area, and even though they are all quite rational. A single rational degree of belief on which all rational human beings should agree seems to be a myth.

So much for the logical interpretation of probability, but the subjective view of probability does not seem to be entirely satisfactory either. Degree of belief is not an entirely personal or individual matter. We very often find an individual human being belonging to a group which shares a common outlook, has some degree of common interest, and is able to reach a consensus as regards its beliefs. Obvious examples of such groups would be religious sects, political parties, or schools of thought regarding various scientific questions. For such groups the concept of intersubjective probability seems to be the appropriate one.

To sum up then, Keynes, and other defenders of the logical interpretation of probability, were wrong to think that there is a single rational degree of belief in a conclusion *c* held by all rational human beings who have the same evidence and background knowledge. On the other hand, Ramsey's account suggests that degrees of belief are more individual and personal than is really the case. We often find that a particular individual belongs to a group whose members have some degree of common interest, share a common outlook, and are able to reach a consensus regarding beliefs. These groups may be small or large, but usually they fall short of embracing the whole of humanity. The intersubjective probability of such a group is thus intermediate between a degree of rational belief (the early Keynes) and a degree of subjective belief (Ramsey).

One final point to note is that while the early Keynes of the *Treatise* [1921] held a logical view of probability, this may not have been true of the later Keynes of the *General Theory* [1936]. The matter is hard to judge since Keynes does not explicitly discuss the problem of interpreting probability in his later economic writings. Nonetheless, as I have argued in my [1988b], there are indications that Keynes changed his mind about probability under the influence of Ramsey, and that the concept of probability which implicitly underlies his *General Theory* [1936] is more like that of intersubjective probability than that of logical probability.

6 THE PROBABILITIES NEEDED FOR CONFIRMATION THEORY

What probabilities are in fact needed for confirmation theory? This question is not altogether easy to answer for there are a number of competing confirmation theories, and the role of probability is different in these different approaches. Let us start, however, by considering Bayesianism which is perhaps the most widely held version of confirmation theory. If we write the degree of confirmation of h given e as $C(h,e)$, the fundamental claim of the Bayesians is that $C(h,e)$ is a probability function, that is to say that it satisfies the ordinary axioms of probability, or, in symbols, $C(h,e) = P(h,e)$. The Bayesians further claim that changes in confirmation in the light of new evidence take place according to Bayes' theorem, *i.e.*

$$P(h,e) = \frac{P(e,h)\,P(h)}{P(e)}$$

A consideration of the right-hand side of Bayes' theorem shows that a Bayesian has to consider, and so interpret, at least the following three probabilities: $P(e,h)$, $P(e)$, and $P(h)$. Strictly speaking these probabilities should be relativized to some background knowledge k, and so should be written $P(e,h\&k)$, $P(e,k)$ and $P(h,k)$. When consideration of the background knowledge is not essential, k can be omitted in order to achieve a greater simplicity of notation.

Let us next consider non-Bayesian approaches to confirmation theory, of which there are several. Some of these do not involve probability at all. For example Cohen has argued ([1977], Chapter 15, pp. 188–98) that $C(h,e)$ is neither a probability nor even a function of probabilities. On his approach the problem of interpreting probability in confirmation theory would not arise. However, some anti-Bayesian confirmation theorists do introduce probability in part of their account of confirmation. This is certainly the case with one of the leading anti-Bayesians: Popper. Popper has always argued against Bayesianism. His own approach to the theory of confirmation (or corroboration as he prefers to call it) is based on the following *Principle of Severe Testing*:

> . . . it is not so much the number of corroborating instances which determines the degree of corroboration as *the severity of the various tests* to which the hypothesis in question can be, and has been, subjected (Popper [1934], Section 82, p. 267).

Popper does not think that degree of corroboration in this sense is a probability. However, when he comes to the question of estimating the severity or goodness of a test, he does make use of probabilities. Thus he considers the expression $P(e,h) - P(e)_1$ and says:

> This means that in order to find a *good* test-statement *e*—one which, if true, is highly favourable to *h*—we must construct a statistical report *e* such that (i) *e* makes *P*(*e*,*h*)—which is Fisher's 'likelihood' of *h* given *e*—large, *i.e.* nearly equal to 1, and such that (ii) *e* makes *P*(*e*) small, *i.e.* nearly equal to 0 (Popper [1959], Appendix *ix, p. 410).

So writing in background knowledge *k*, we see that Popper considers two probabilities *P*(*e*,*h*&*k*), and *P*(*e*,*k*). These are in fact the first two of the three probabilities handled by the Bayesians.

Let us now consider how these various probabilities are interpreted. The Bayesians divide into two camps. The logical Bayesians adopted the logical interpretation of probability, *e.g.* Jeffreys [1939]. The subjective Bayesians adopt the subjective interpretation of probability, *e.g.* De Finetti [1970]. Popper firmly repudiates the subjective view of probability writing: 'These remarks should not be taken to mean that I am prepared to accept *any* form of the subjective interpretation; . . .' (Popper [1959], Appendix *ix, p. 407). Instead Popper adopts a form of the logical interpretation of probability which, however, differs from that of Johnson, Jeffreys and Keynes. To begin with, since Popper is not a Bayesian, he does not identify the logical probability of *h* given *e* (*P*(*h*,*e*)) with the degree of corroboration of *h* given *e* (*C*(*h*,*e*)). Moreover he identifies 'degree of rational belief' (or rather 'degree of rationality of a belief' (as he prefers to say) with degree of corroboration rather than with logical probability. As he puts it himself:

> . . . while it is a mistake to think that probability may be interpreted as a measure of the rationality of our beliefs . . ., degree of corroboration may be so interpreted. As to the calculus of probability, it has a very large number of different interpretations. Although 'degree of rational belief' is not among them, there is a *logical interpretation* which takes probability as a generalization of deducibility (Popper [1959], Appendix *ix, pp. 414–5).

I now turn to an exposition of my own views on the question. As explained in the previous section, I do not think that it is possible to give a satisfactory logical interpretation of the usual calculus of probability. This is largely because of Ramsey's criticisms of the logical view. Admittedly these criticisms were directed against Keynes, but they do seem to me to apply to other versions of the logical interpretation such as Popper's. I do, however, regard subjective betting quotients as constituting a perfectly valid interpretation of the probability calculus, but I do not think this is the appropriate interpretation for the probabilities in confirmation theory. The problem is that these probabilities are not a matter of individual taste, and do not vary in an arbitrary manner from scientist to scientist. On the contrary there is usually a high degree of consensus in the scientific community as to whether a given theory *T* is well-confirmed or badly-confirmed by the available evidence. This suggests that the probabilities in confirmation theory should be interpreted as intersubjective probabilities. If the hypotheses ($h_1, h_2, \ldots$) and the evidence ($e_1, e_2, \ldots$) involved belong to some branch of science, the social group can usually be taken as the group of scientists working in that field. As we shall see, this can make the social group too large for certain purposes. We may sometimes have to take account of the division of the group into different schools of thought. It may even, on occasion, be important to consider the subjective probabilities of dissident scientists who reject the prevailing consensus. However, we shall leave these refinements for later, and begin by considering the intersubjective or consensus probabilities of the social group of scientists working in a particular branch of science.

This approach ties in very well with the ideas of Fleck (see his [1935]). Fleck holds that scientific knowledge is carried and developed by what he calls a 'thought collective'. This he introduces as follows:

> If we define 'thought collective' as *a community of persons mutually exchanging ideas or maintaining intellectual interaction, we will find by implication that it also provides the special 'carrier' for the historical development of any field of thought, as well as for the given stock of knowledge and level of culture* (Fleck [1935], p. 39).

Fleck goes so far as to say: '. . . the true creator of a new idea is not an individual but the thought collective' (Fleck [1935], p. 123). Fleck's argument is roughly the following. Suppose a new idea is created between t_1 and t_2. If we examine what went on in the thought collective during that time period, we shall find that no particular individual succeeded in formulating the idea in its final form. Instead we shall discover that different individuals put foward different fragments of the idea mixed up with what was later seen to be error and confusion. Eventually, however, after long discussions within the collective, the new idea takes on its definitive shape.

It may be that Fleck here underplays the importance of individual contributions,[4] but his opinion is a useful corrective to the view of science as the creation of the great geniuses. However outstanding the talent of an individual scientist, he or she will always owe a great deal to his or her thought collective. Fleck does make some concession to the individual when he writes of 'The three factors involved in cognition—the individual, the collective, and objective reality (that which is to be known) . . .' (Fleck [1935], p. 40). Note that these three factors correspond to the three interpretations of probability introduced earlier: the subjective, the intersubjective, and the objective.[5]

We can now consider in more detail the three probabilities characteristically involved in confirmation theory, *i.e.* $P(e,h\&k)$, $P(e,k)$, and $P(h,k)$. Fleck's theory leads to a satisfactory account of the background knowledge *k*. This is just the set of propositions accepted by the scientific thought collective at the time, excluding of course the hypothesis *h* and the evidence *e* under consideration. Note that *k* may be considerably more extensive than the knowledge possessed by any individual scientist. Since the scientific thought collective mutually exchanges ideas and maintains intellectual interaction, if one member has a piece of relevant knowledge which the others lack, he or she can communicate it to the others. Similarly the logical and mathematical powers of the thought collective will normally exceed those of any of its members. If anyone makes a logical mistake, this is characteristically exposed and corrected by someone else, and even the finest mathematicians make the occasional logical blunder.

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[4] Fleck's own highly individual contribution to philosophy of science somewhat contradicts his own theory that new ideas are the product of a thought collective. On the other hand the poor reception initially given to Fleck's ideas is in accordance with Fleck's theory of thought collectives. The case of Fleck in relation to the thought collective of philosophers of science is rather similar to that of Frege in relation to the thought collective of logicians.

[5] They also correspond roughly to Popper's Worlds 2, 3 and 1. See Footnote 2.

Within a scientific thought collective, Fleck distinguishes what he calls the 'esoteric circle' from the 'exoteric circle'. The esoteric circle consists of those scientists who are the leading experts, who make the biggest research contributions, and who perhaps may be candidates for a Nobel prize. The exoteric circle consists of the remaining more average scientists. Now Fleck's very interesting view is that the scientific thought collective is in fact democratic in character, and that the exoteric circle dominates the esoteric circle. Here is how Fleck puts it:

> The general structure of a thought collective consists of both a small esoteric circle and a larger exoteric circle, . . . But the initiated are by no means independent. They are more or less dependent, whether consciously or subconsciously, upon 'public opinion', that is, upon the opinion of the exoteric circle . . . The esoteric circles thus each enter into a relation with their exoteric circles known in sociology as the relation of the elite to the masses. If the masses occupy a stronger position, a democratic tendency will be impressed upon this relation. The elite panders, as it were, to public opinion and strives to preserve the confidence of the masses. This is the situation in which the thought collective of science usually finds itself today . . . The . . . democratic . . . form must lead to the development of ideas and progress (Fleck [1935], pp. 105–6).

At first sight the claim that the thought collective of science is democratic in character seems strange if not paradoxical. Let us take a typical scientific research institute. At the top there is director. The comes a graded hierarchy of full professors, associate professors, assistant professors (to use the American terminology), while, at the bottom, there is an underprivileged and insecure group of research assistants. The system has a built-in mechanism of exploitation known as the joint paper which enables those higher up the hierarchy to take the credit for the bright ideas of the research assistants. All this bears more resemblance to the medieval feudal system of King, Dukes, Barons and toiling peasants, than to any egalitarian and democratic social structure.

Yet there is some truth in what Fleck says all the same. Evidence for Fleck's view is found in the citation studies which show that those who win the Nobel prizes are usually those who are most often cited by scientists working in the same field. Recruitment to the elite of Nobel prizewinners certainly does not take place by any explicitly democratic process, and yet the views of the broad mass of ordinary working scientists do seem to be taken into account by some indirect mechanism.

It is interesting to note, as Bernard Cohen has pointed out, that the situation regarding Nobel prizes is very different in literature from what it is in science.

> Although almost all of the recipients of the Nobel Prize for physics, chemistry, and medicine have merited an award, the same can hardly be said for literature. Many awards have been made to relatively unknown and uninfluential writers who have soon thereafter been forgotten. Those who have failed make up a list of enormous distinction (like the 'salon des refusés'), including Guillaume Apollinaire, W. H. Auden, Berthold Brecht, Herman Broch, L.-F. Céline, Joseph Conrad, Hendrik Ibsen, Henry James, James Joyce, D. H. Lawrence, Federico Garcia Lorca, Stéphane Mallarmé, Vladimir Maykovsky, Robert Musil, Vladimir Nabokov, Ezra Pound, Marcel Proust, Rainer Maria Rilke, August Strindberg, Leo Tolstoy, Paul Valéry, and Virginia Woolf. These 'losers' constitute a veritable 'Who's Who' of modern literature (Cohen [1985], p. 576).

The explanation in my view is that the democratic tendency which Fleck noted in the scientific thought collective (despite its neo-feudal structure) is absent in the thought collective of literature. Those in powerful positions in the literary world do not take account of the views of the mass of ordinary readers of serious literature.

This last point relates to some interesting views of Bloor expressed in his [1978] which are in turn based on Douglas [1973]. Bloor takes from Douglas the classification of Societies in terms of two theoretical dimensions 'grid' and 'group'. A society is high group if social interactions are very strong and compelling as when people work together, take their leisure together, intermarry, and live in the same neighbourhood. A society is low group if personal pressures can be evaded by changing jobs, houses, or friends. A society is high grid if there are extended gradations of rank, of a kind associated with varying rights and duties and expected kinds of behaviour. An army or a bureaucracy is high grid, as was the feudal system.

Bloor applies this classification in an ingenious fashion to the communities of research mathematicians and scientists. Without attempting to summarize his detailed investigations, I shall pick up one point which is relevant to the present discussion. Bloor writes:

> We must ask what social forms exert a pressure towards innovation and novelty and encourage transactions across the boundaries of existing classificatory schemes, dissolving them in change? Where is discontinuity more desired than regularity? Where can mistakes be tolerated and risks taken? (Bloor [1978], p. 256).

The answer is, of course, low group/low grid societies, which are thus the societies most conducive to progress in science.

If this is correct, then we can see that the present 'neo-feudal' organization of science is actually a brake on efficiency and slows down progress in scientific research. Objective need may therefore force a relaxation of the authoritarian, hierarchical social structure, and allow a counter-vailing democratic tendency to manifest itself in science.

Returning now to questions of confirmation, we can say that scientific theories although perhaps mainly proposed by members of the esoteric circle are judged by the scientific thought collective as a whole, and not just by the leading scientific experts. The intersubjective probabilities in confirmation theory should then apparently be the consensus probabilities of all the scientists working in the branch of science in question. But do we find such a general consensus in practice? This is the question which we must next consider.

The general consensus of a scientific thought collective is broken by two factors, namely (i) the existence of scientific rebels or dissidents who reject the received opinions and have unusual ideas of their own; and (ii) the existence of different schools of thought within the collective. We will deal with these in turn.

As for the scientific rebels or dissidents, the appropriate interpretation of probability is clearly the subjective one. It should be stressed that dissidents are just as valuable in the scientific community as they are in the political community, and this is a strong argument that subjective probabilities are needed as well as intersubjective ones. The ideas of an isolated dissident may become the basis of a new theory which brings remarkable progress, and which indeed becomes itself in due course the established orthodoxy. Of course this does not happen very often. Most dissidents are judged at the time to be cranks, and this judgement is usually endorsed by history. But even if a dissident's ideas are eccentric and crankish, and never become (even in a modified form) generally accepted, they still can be very useful for science. At the very least they can undermine to some small extent the dogmatism with which received ideas are often held, and they can in the process suggest weaknesss in orthodox theories which may then be resolved in ways quite different from those suggested by the dissident.

Let us next consider the existence of different schools of thought within the collective. These might be based on different paradigms in the sense of Kuhn [1962], or consist of scientists working on different research programmes in the sense of Lakatos [1970]. As far as intersubjective probabilities are concerned, these can sometimes be taken as the consensus probabilities of the whole relevant scientific thought collective, while, in other contexts, they are only the consensus probabilities of one of the schools of thought within the collective. This can be illustrated by returning to our three probabilities $P(e,h\&k)$, $P(e,k)$ and $P(h,k)$.

The first two of these probabilities can, in my view, be taken as intersubjective probabilities of the whole relevant scientific thought collective. $P(e,h\&k)$ is just the likelihood of h given e, and can usually be calculated in a quite unproblematic manner. $P(e,k)$ is the probability of obtaining a particular observation or experimental result relative to the background knowledge k. This seems to me to be something on which most scientists could agree irrespective of the research programme on which they happen to be working.

When we turn to the third probability $P(h,k)$, the situation is different. Scientists working on different research programmes are likely to judge the plausibility of a hypothesis on background knowledge differently. Indeed it is just such differences in judgement which give rise to different research programmes. Suppose the scientists working on research programme R_1 are trying to develop the hypothesis h_1 while those working on R_2 are trying to develop h_2. The R_1 scientists will surely judge h_1 to be more plausible than h_2, while the R_2 scientists will judge h_2 to be more plausible than h_1. Thus if the probabilities $P(h,k)$ can be introduced at all, they should be considered as intersubjective probabilities of a school of thought within the relevant scientific collective. However, there are some doubts as to whether the probabilities $P(h,k)$ can be introduced in any sensible fashion, since there are arguments which suggest that $P(h,k)$ must always be zero for any universal hypothesis.[6]

These considerations constitute, in my opinion, an argument in favour of Popper's approach to confirmation theory. We want, as far as possible, to ensure that our confirmation function is based on intersubjective probabilities which are consensus probabilities of the whole relevant scientific thought collective. In this way we can achieve general agreement in judgement as to how the competing research programmes are progressing. But this means that we should try to confine ourselves to probabilities like $P(e,h\&k)$ and $P(e,k)$, and try to avoid the prior probabilities $P(h,k)$ of the Bayesians.

7 AN OBJECTION: THE GAME AGAINST NATURE

It is time now to consider an important objection against the use of the betting approach in confirmation theory. This objection was originally made against the use of subjective probabilities. However, it is really a difficulty concerned with the use of the Dutch Book argument, and so applies, just as forcibly, to intersubjective probabilities. Indeed, as we shall see, the problem is worse for the intersubjective case.

In the Dutch Book argument, we consider B, or $\mathscr{B} = (B_1, B_2, \ldots, B_n)$ betting against A. Now suppose we are dealing with confirmation theory, and the Bs are scientists. It looks as if we have to consider A as 'nature' and the scientists as playing a game against nature. But is it now reasonable to consider nature as a cunning bettor trying to win money off the scientists?

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[6] There is a discussion of these arguments in my ([1988a], pp. 192–5). The most convincing argument, in my view, is based on betting, and is expounded in Cohen ([1970], Chapter 4, pp. 129–30).

The difficulty was first pointed out by Putnam who wrote:

> For we are not playing against a malevolent opponent but against nature, and nature does not exploit such 'incoherencies' . . . (Putnam [1963], p. 303).

It was endorsed by Lakatos who argued:

> The correct conclusion is that it is irrational to base our theory of rationality on the manichean assumption that if we do not arrange our bets (or degrees of belief) probabilistically an evil power will catch us out by a shrewdly arranged system of bets (Lakatos [1968], p. 159).

As has been pointed out to me,[7] the situation is even worse as regards intersubjective probabilities, for here we have apparently got to imagine nature betting in different ways on the occurrence of the *same* event. In the light of this difficulty, it does not seem to me possible to introduce intersubjective probabilities by considering the scientific thought collective as playing a game against nature. Another approach is needed.

The approach which I shall in fact adopt is based on Ramsey's original and highly subtle discussion of the problem of measuring belief ([1926], pp. 69–73). I will begin with a brief recapitulation of some points of Ramsey's.

Ramsey argues in favour of connecting the degree of a belief with the actions performed on the basis of that belief. As he says: '. . . the degree of a belief is . . . the extent to which we are prepared to act on it' (Ramsey [1926], p. 71). However, this connection is dispositional and the actions may be hypothetical rather than real. '. . . it is not asserted that a belief is an idea which does actually lead to action, but one which would lead to action in suitable circumstances; just as a lump of arsenic is called poisonous not because it actually has killed or will kill anyone, but because it would kill anyone if he ate it' (Ramsey [1926], p. 71). Ramsey also introduces an interesting comparison between measuring belief and measuring electric intensity, for, as he says '. . . the electric intensity at a given point is the force which would act on a unit charge if it were placed at the point' (Ramsey [1926], p. 73).

Let us develop this comparison a little. The beliefs of an individual, or social group, X say, will affect the way X behaves in all sorts of different ways. Similarly the strength of an electric field at various points will affect the behaviour of various kinds of physical bodies placed at those points. To render the electric field measurable, we have to select one particular observable effect of the field (in this case the force on a unit charge placed at the point in question of the field), and use that as our measure. This does not of course mean that whenever we assign a value to an electric field at a given point, we will always place a unit charge at that point in order to obtain the value. In fact, this is hardly ever done. Usually some more indirect method of estimating electric intensity is employed. Nonetheless the definition in terms of force on a unit charge is important as the basis of the measurement process.

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[7] By Prof. M. B. Hesse (Private Correspondence).

Exactly the same considerations apply to the measurement of belief by betting. X's beliefs affect X's behaviour in all sorts of ways, but to render a belief measurable, we have to select just one particular observable effect of this belief. The effect in question is the choice of betting quotient which X would make if forced to bet under the conditions specified earlier; and we use this effect as our measure.

Turning now to the beliefs of scientists, we see that we do not have to introduce the concept of a game against nature, but only that of a hypothetical experiment, which could be, but need not be, carried out by a perfectly human experimenter A, or group of experimenters $\mathscr{A}$. If we want to know the belief that a scientific thought collective $\mathscr{B}$ has in the outcome of a hitherto unperformed experiment (say), we could employ an experimental psychologist (or perhaps sociologist) A to bet with all the members of $\mathscr{B}$ on the outcome in the specified manner, while, of course, allowing the members of $\mathscr{B}$ to discuss the appropriate betting quotient among themselves. Such an experiment is possible, but it would usually be easier to estimate the collective's intersubjective probability by more indirect means—just as is done in the case of electric intensity. Nonetheless the definition of belief in terms of betting is important as the basis of the measurement process.

The theory of electric fields with its definition of electric intensity at a particular point is one of the most successful and important theories of physics. The real questions regarding subjective and intersubjective probabilities are these. Can a theory of human belief with a definition of degree of belief in terms of the Dutch Book argument become an important and successful theory of psychology (or perhaps sociology)? Can such a theory help to explicate the beliefs of scientists, and their judgements of confirmation? To me the outlook for a theory of this type seems to be promising, but the issue is far from being decided as yet.

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A Practical Example of Grue

F. M. AKEROYD

ABSTRACT

This article describes a practical example of the predicate grue, examining the economic relationship between the percentage rate of unemployment and the percentage change of money wage rates known as the simple Phillips Curve which exhibited regular behaviour before 1969 and erratic behaviour thereafter. It is proposed that such practical examples of grue from the real world be re-described as **regulatic**, *i.e.* regular before time t and erratic thereafter. In the instance of a scientific model or theory being falsified it is proposed to describe the predicate as **tralse**, *i.e* true before time t and false thereafter. The example to be discussed is a stronger example since entities in the real world started to behave in an extremely unpredictable fashion.

In his book *Fact, Fiction and Forecast* Nelson Goodman [1955] introduced the concept of grue. He gave as an example the case of emeralds: since all observed emeralds have been green up to the present day we assume this evidence supports the hypothesis 'In the future all emeralds will be green'. However, he points out that this evidence also supports the contrary hypothesis that all present day emeralds are 'grue', *i.e.* green in the past but blue in the future. This apparent paradox has led to much philosophical debate and talk of a 'new riddle of induction': a riddle the subtlety of which is not always appreciated by beginning students of philosophy.

Later, in a short paper entitled 'Safety, Strength and Simplicity' Goodman [1961] gave a less esoteric example:

> Suppose our evidence tells us just that every examined A is a B, and suppose hypothesis H_1 affirms in addition that every (or even some one) other A is a B. Then hypothesis H_2 affirming that every A is a B and that every (or the particular one) other A is not a B, likewise conforms to the stated evidence.

In my opinion a more convincing predicate to project to beginning students would be that of 'regulatic', referring to phenomena which exhibit regularities before time t and erratic behaviour thereafter.

A convincing example of the concept of regulaticity arose in 1969 when certain economic indicators started to fail to follow the neat pattern predicted by a theoretical model known as the Phillips Curve. This model, proposed in

1958, had been enthusiastically adopted by economic advisers to Western Governments and used with great success in the period 1960–9. The model suggested that there was a trade-off between the % unemployment and the % annual rate of wage inflation in a free market economy: for example the Government could choose between (say) 1% wage inflation and 3½% unemployment or 8% wage inflation and 1% employment or any intermediate pair of variables which lay on the curve.

In the opening paragraph of his paper Phillips [1958] stated:

> The relation between unemployment and the rate of change of wage rates is therefore likely to be highly non-linear.

and later:

> The purpose of the present study is to see whether statistical evidence supports the hypothesis.... The periods 1861–1913, 1913–48 and 1948–57 will be considered separately.
>
> The form of equation chosen was
>
> $$\begin{aligned} y+a &= bx^c \\ \text{or } \log(y+a) &= \log b + c\log x \end{aligned}$$
>
> where y is the rate of change of wage rates and x is the percentage unemployment. The constants b and c were estimated by least squares using the values of y and x corresponding to the crosses in the four intervals between 0 and 5 per cent unemployment, the constant a being chosen by trial and error to make the curve pass as close as possible to the remaining two crosses in the intervals between 5 and 11 per cent unemployment. The equation of the fitted curve is
>
> $$\begin{gathered} y + 0{\cdot}900 = 9{\cdot}638x - 1{\cdot}394 \\ \text{or } \log(y + 0{\cdot}900) = 0{\cdot}984 - 1{\cdot}394 \log x \text{ (see Figure 1)} \end{gathered}$$
>
> The statistical evidence in Sections II to IV above seems to support the hypothesis stated in Section I, that the rate of change of money wage rates can be explained by the level of unemployment, and the rate of change of unemployment, except in or immediately after those years in which there is a sufficiently rapid rise in import prices to offset the tendency for increasing productivity to reduce the cost of living. The conclusions are of course tentative.

It is a matter of history that after 1969 the relationship broke down badly and the data stubbornly refused to fit the curve. Various explanations were offered involving the rise of trade union power, rise of multi-national corporations, changes in workers' inflationary expectations and the economic consequences of the Oil Crisis. Fudge factors were introduced to demonstrate that the data fitted *ad hoc* modified Phillips curves. However the verdict of the economists is neatly summed up by Samuelson [1986] who states:

> The theory of the simple and stable Phillips curve is dead.

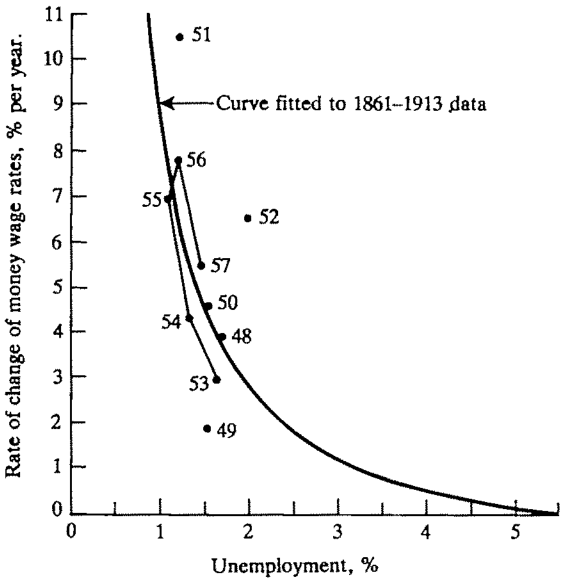

FIGURE 1

To many economists the simultaneous increase in the rate of wage inflation and the level of unemployment which led to the breakdown of the Phillips Curve after 1969 was almost as surprising as all emeralds being observed to be blue after 1969. This is why the behaviour of Western Economies after 1969 is such a strong example from the real world of Nelson Goodman's concept of grue (see Figure 2).

In the field of Chemistry the synthesis of a new substance which reacts with a known substance previously believed to be unreactive is not uncommon. An idea which was true up to the year 1300 (approx.) and false thereafter was the immutability of the metal gold and consequently many strange ideas concerning its magical properties had occurred in times prior to this date. Around the year 1300 chemists found that gold would dissolve in 'aqua regia', a concoction containing concentrated forms of nitric acid and hydrochloric acid which does not occur naturally and had not been used before (Leicester [1956]).

The mechanism is thought to be that traces of free chlorine (an element not isolated until the 1780s) are formed which then attack the gold and trigger off the dissolution. So the idea of the immutability of gold was tralse, true before 1300 and false thereafter.

$$2HNO_3 + 2HCl \rightarrow 2H_2O + 2NO_2 + Cl_2$$

$$2Au + 3Cl_2 \rightarrow 2AuCl_3$$

$$AuCl_3 + HCl \rightarrow HAuCl_4$$

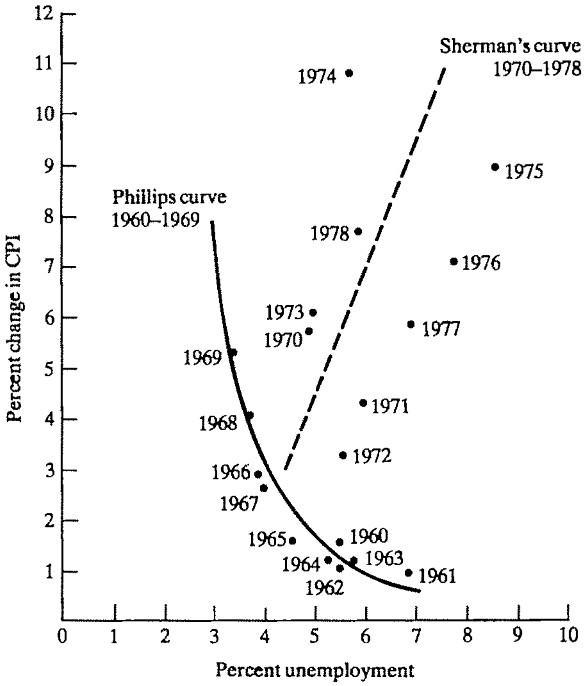

FIGURE 2

Another example which might be thought to support the concept of tralsity is that of the unreactivity of the noble gases, isolated in the nineteenth century, which survived many determined attacks to make them react until 1962 when Neil Bartlett [1962] mixed the colourless gas xenon with the violet vapour of the recently synthesized platinum hexafluoride and obtained a yellow solid. Because platinum hexafluoride was a novel compound it would appear that the concept of the inertness of the noble gases and the associated concept of the exceptional stability of the Electronic Octet had been demonstrated to be tralse but this apparent tralsity only lasted for three months. In August 1962 Claasen, Selig and Malm [1962] announced that they had successfully reacted xenon with elemental fluorine, a reaction which had been tried before on several occasions without success. This means that the concept of the unreactivity of the noble gases was not tralse before May 1962 but falsely believed to be true, an important but subtle distinction,[1] since chemists could have used the same procedures as Claasen and his co-workers.

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[1] A referee made the point that it could be argued that the belief concerning the immutability of gold was also an example of some-thing falsely believed to be true and not, as I suggested, an

example of tralsity. It is true that the *components* of aqua regia were all well known chemicals and that chemists *could* have made up the mixture in the correct proportions at an earlier date.

However due to the limitations of glass technology it is unlikely that the distilled nitric acid/sulphuric acid mixture was concentrated enough pure enough to act (on addition of ammonium chloride) as a successful solvent for gold. Similarly there were technical problems prior to 1960 in synthesising and handling pure platinum hexafluoride.

However in the period 1895–1940 several chemists proposed that pure xenon (available) could probably react with pure fluorine (available) and made determined but unsuccessful efforts using techniques very similar to those used subsequently by Claasen and co-workers. In this account I am taking the technical apparatus available to a chemist as 'given', but arguing that if a group of chemical reagents *could* have been mixed together in the correct proportions with appropriate catalysts to produce a new compound at some previous date then they *should* have been mixed together.

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Bell's Inequalities and Quantum Non-separability

S. V. BHAVE

ABSTRACT

The separable hidden variables theory (Bhave [1986]) of Aspect's [1982] four single channel polarizers is developed further to consider possible modified Aspect's experiment with four double channel polarizers. It is shown that Aspect's commutator is not a truly stochastic commutator, and that until such a truly stochastic commutator is devised, experiments based on Bell's inequalities (like those of Aspect's) cannot be adequate experimental tests of quantum non-separability.

1 *A modified Aspect's experiment with four double channel polarizers*
2 *The physical characteristics of Aspect's commutator*
3 *The 'rapport' between commutator, polarizers and the source of light*
4 *Limitations of experiments based on Bell's inequalities*
5 *Questions of plausibility*

I A MODIFIED ASPECT'S EXPERIMENT WITH FOUR DOUBLE CHANNEL POLARIZERS

In a previous paper (Bhave [1986]) it has been shown that a separable hidden variables theory explains the results obtained in Aspect's [1976, 1982] four single channel polarizers experiment. We now extend the theory to a possible modified Aspect's experiment with four double channel polarizers in which a photon 1 produced first by a cascading atom meets a commutator C_A and is directed by it stochastically to either polarizer A_1 (with channels in orientations a_1 and $a_2 = a_1 + \pi/2$) or to polarizer A_2 (with channels in orientations a_3 and $a_4 = a_3 + \pi/2$), and the corresponding photon 2 produced next, flying in the opposite direction is directed by commutator C_B to either a polarizer B_1 (with channels in orientations b_1 and $b_2 = b_1 + \pi/2$) or to polarizer B_2 (with channels in orientations b_3 and $b_4 = b_3 + \pi/2$). If the channels of the polarizers A_1 and A_2 in orientations a_1, a_2, a_3, a_4 are called channels a_i ($i = 1,2,3,4$) and those of the polarizers B_1 and B_2 are called channels b_j ($j = 1,2,3,4$), the quantum theory prediction of the coincidence counts for the events 'photon 1 passes through channel a_i and photon 2 passes through channel b_j' are $R(a_i,b_j)=R_0(a_i,b_j)\cdot(1/2)\cdot\cos^2(a_i-b_j)$ (in the 0–1–0 case) where $R_0(a_i,b_j)$ is the rate monitored by the photon detectors in the absence of the polarizers.

Let v_1 be the variable with value ranging from 0 to 2π, expressing the plane polarization orientation of the beam of photons to which a photon belongs, and v_{2x} be the variable with possible values 0 or 1, such that if for the photon $v_{2x}=0$, on meeting a polarizer with a channel in orientation x, it will pass it (through that channel), and if $v_{2x}=1$, it will not pass it through that channel. In the experiment with four double channel polarizers we assume that in addition to some value of the variable v_1, every photon 1 is produced with some one of the following four sets of the variable v_{2ai} values:

$$v_{2a1}=0;\ v_{2ai}=1 \text{ for } i=2,3,4 \tag{1}$$

$$v_{2a2}=0;\ v_{2ai}=1 \text{ for } i=1,3,4 \tag{2}$$

$$v_{2a3}=0;\ v_{2ai}=1 \text{ for } i=1,2,4 \tag{3}$$

$$v_{2a4}=0;\ v_{2ai}=1 \text{ for } i=1,2,3 \tag{4}$$

We assume that a photon 1 is created with v_1 value lying between y and $y+dy$ with probability $dy/2\pi$, and with any one of the four sets of values for v_{2ai} with equal probability 1/4. The corresponding photon 2 has $v_1=a_i$ where for the photon 1, $v_{2ai}=0$, and with $v_{2bj}=0$ with probability $\cos^2(a_i-b_j)$ ($j=1,2,3,4$). If the commutator directs photons 1 to polarizers A_1 or A_2 stochastically (that is independently of their v_1 as well as v_{2ai} values), we must have for the coincidence counting rates

$$R(a_i,b_j)=R_0(a_i,b_j)\cdot(1/4)\cdot\cos^2(a_i-b_j)$$

which is only half the rate predicted by quantum theory. With still larger number of polarizers $A_1, A_2, \ldots A_n$, the coincidence counting rates would be still smaller.

However, as we shall see in the next section, with the actual commutator used by Aspect (which cannot work stochastically because of its physical characteristics), the predictions of the separable hidden variables theory must coincide with those of quantum theory.

2 THE PHYSICAL CHARACTERISTICS OF ASPECT'S COMMUTATOR

Aspects [1976, 1982] commutator consists of acoustic standing waves in water, acting as adjustable gratings. The deflected beam is the diffracted beam; and so the polarization of the incident beam and that of the transmitted and diffracted beams directed to the polarizers A_1 and A_2 must be the same. Since the incident beam of photons 1 can be experimentally verified to be unpolarized light, the beams directed to polarizers A_1 and A_2 must also be unpolarized light; that is half the photons in the beam meeting any polarizer must pass through one channel of the polarizer, and the remaining half must pass it through the other channel. This will only be possible if the commutator directs *all* photons 1 with $v_{2al}=0$ to the polarizer containing the channel a_l. Moreover, since the intensities of the two beams of light meeting polarizers A_1 and A_2 vary periodically, their sum being always equal to that of the incident beam, (Bauza [1974], Mahajan [1974], Yariv [1975]), the source of light must be producing photons 1 with particular v_{2al} values at a rate varying periodically; that is the source of light, the commutator and the polarizers work as if they are engaged in a 'conspiracy' to keep the incident beam, and the two beams directed to the two polarizers, experimentally verifiably unpolarized. Aspect [1976] had considered the possibility of such a 'conspiracy'; and to prevent it had proposed steps like changing the modulation rate, or using inhibited detectors which monitor the coincidences only during the short intervals in each acoustic period when the light is almost wholly transmitted or almost wholly deflected. None of these steps is likely to produce any different results, since with any modulation rate, and over every finite time interval composed of a sufficient number of short intervals during which the inhibited detectors work, the light incident on the commutator, and the light incident on each polarizer must continue to be unpolarized light. In fact this 'conspiracy' between the source and the measuring instruments is something we come across ever so often whenever a sufficiently large number of photons produce the phenomena of polarization, interference and diffraction of light, phenomena that are so much easier to understand or picturize in the wave theory of light.

With such selective direction of photons 1 to polarizers A_1 and A_2, the predictions of the separable hidden variables theory must coincide with those of quantum theory.

3 THE 'RAPPORT' BETWEEN COMMUTATOR, POLARIZERS AND THE SOURCE OF LIGHT

It will be observed that in the theory of Aspect's [1982] experiment with four single channel polarizers, it was only necessary to assume a 'rapport' between the polarizers and the source of light; in the theory of modified Aspects experiments we need to assume a more complicated rapport between the polarizers, the commutator and the source of light. This is because in Aspect's [1982] experiment only two types of photons 1 were assumed to be produced; in the modified Aspect's experiment discussed in this paper we have to assume four types of photons 1. In each case, the type of rapport assumed (or lack of rapport assumed) has to be such as would ensure that the working of the polarizers and commutator is consistent with their known physical characteristics:

(1) Of the unpolarized light incident on any polarizer, a single channel polarizer will pass only half the light and a double channel polarizer will pass the whole of the light, and
(2) the commutator will absorb no part of the incident unpolarized light and the deflected and transmitted beams will be unpolarized light. Thus the more complicated 'rapport' that has to be assumed in the modified Aspect's experiments is the price we pay for preserving in the theory consistency with the known physical characteristics of the polarizers and the commutator.

It is not suprising that in a more complicated experimental situation, different parts of the experimental apparatus appear to have established a more complicated 'rapport' with each other. Consider monochromatic light from a source of light passing through two slits in a screen and producing a diffraction pattern on a receiving screen placed beyond the slits. When the distance between the two slits is large, each slit seems to have established a rapport with the source of light in that it produces photons of different dynamical properties (in the sense that they impinge at different places on the receiving screen) in different proportions. Each slit appears to have established such rapport with the source independently of the other slit. But when the distance between the two slits is made sufficiently small, a new type of rapport appears to be established between the two slits and the source of light such that a very different diffraction pattern (very different from the simple superposition of the two diffraction patterns of the individual slits) is now produced.

4 LIMITATIONS OF EXPERIMENTS BASED ON BELL'S INEQUALITIES

We have at present no idea how to devise a truly stochastic commutator. It is clear that until such a truly stochastic commutator can be devised, experiments (like those of Aspect) based on Bell's m̄ inequalities can be useful to clarify various details of the photon picture of the phenomena of polarization and diffraction of light; but they cannot be adequate experiments for an experimental test of quantum non-separability.

5 QUESTIONS OF PLAUSIBILITY

Some thinkers (Bell [1966], Shimony [1984]) have raised the question of the 'physical acceptability' or 'plausibility' of separable hidden variables theories. Any theory which postulates hidden variables values for photons can obviously at best be no more 'physically plausible' than the photon picture of light itself. We all agree that neither the photon picture nor the wave picture of light is really an adequate picture of the physical reality behind the optical phenomena. But the primary purpose of developing any separable hidden variables theory is only to explore the possibility of an experimental decision between two physical principles none of which is tied to any particular picture (like the photon or the wave picture) of physical reality; namely the Einstein principle of separability (Aspect [1976]) and the principle of quantum non-separability. It would appear that a separable hidden variables theory can be properly judged only on the ground of its logical consistency, and its ability to point out what possible experiments could, or could not convincingly decide between Einstein's separability, and quantum non-separability.

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DISCUSSION

The 'Old Evidence' Problem

ABSTRACT

This paper offers an answer to Glymour's 'old evidence' problem for Bayesian confirmation theory, and assesses some of the objections, in particular those recently aired by Chihara, that have been brought against that answer. The paper argues that these objections are easily dissolved, and goes on to show how the answer it proposes yields an intuitively satisfactory analysis of a problem recently discussed by Maher. Garber's, Niiniluoto's and others' quite different answer to Glymour's problem is considered and rejected, and the paper concludes with some brief reflections on the prediction/accommodation issue.

1 *Introduction: Glymour's Poser*
2 *And an Answer*
3 *Chihara's Objections*
4 *How the Answer Solves a Problem*
5 *Garber's Answer*
6 *Conclusion: Accommodation and Prediction*

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1 INTRODUCTION: GLYMOUR'S POSER

Clark Glymour [1980] presented subjective Bayesians with the following problem. Suppose a hypothesis *h* is proposed which turns out to explain some already well-known data *e*. Can *h* ever be confirmed by *e*? Intuitively, the answer is that it can. Examples abound, in fact, of *h*s apparently well-supported by such *e*s. One very often mentioned is General Relativity with its allegedly strong support from the data on the annual precession of Mercury's perihelion, data already fifty years old when the field equations of General Relativity were first obtained by Einstein.

The problem for subjective Bayesians is explaining and justifying the attribution of such support. For Bayes's Theorem says that

$$P(h/e)=\frac{P(e/h)P(h)}{P(e)} \tag{1}$$

and the received wisdom is that for subjective Bayesians these probabilities are all relativized to the individual's stock K of contemporary background information. This relativization has the following unpleasant consequence, however. If e is known at the time h is proposed, then e is in K and so $P(e) = P(e/h) = 1$, giving, from (1), $P(h/e) = P(h)$; which means that e gives no support to h.

2 AND AN ANSWER

One answer—and I think the correct one—to Glymour's nasty problem, often called the *Old Evidence problem*, is to deny that when assessing support according to the difference between $P(h/e)$ and $P(h)$, the probabilities should be relativized to K; rather they should *always* be relativized to $K-\{e\}$ (I shall discuss the question of whether this quantity can be consistently defined in a moment). And why? The answer is straightforward. When you ask yourself how much support e gives h, you are plausibly asking how much a knowledge of e *would* increase the credibility of h, which is the same thing as asking how much e boosts the credibility of h relative to what *else* you currently know. The 'what else' is just $K-\{e\}$.

Glymour considered this way of evaluating support in the case in which e is already known, but not favourably. His objection is a curious one, however: it seems to be merely that there is no general procedure for computing $P(e)$ in such cases (p. 87). But the subjective Bayesian will not be disturbed by this observation, because $P(e)$ and $P(h)$ are explicitly regarded as exogenous parameters within his theory, whatever they're relativized to. It may take some exercise of the imagination to evaluate what your degree of belief in h would be were you, counterfactually, not to know e, but there is no reason in principle to think that it can't be done. Indeed, it seems to be done all the time: 'if I hadn't seen him palm that card, I'd think he had paranormal powers', for example.

3 CHIHARA'S OBJECTIONS

While Glymour's objection seems easily answered, it is not the only one to have been brought against the $K-\{e\}$ relativization for the probabilities used in computing support. Chihara's recent objections [1987] turn on the exact nature of $K-\{e\}$. His first objection is as follows: if $K-\{e\}$ signifies merely the set-theoretic subtraction of e from K, and K itself is intended always to be a deductively closed set of sentences, then $K-\{e\}$ will not be well defined if there are other sentences in K entailing e. Nor is there any unambiguous direction concerning which to remove if there are. For if a and b jointly but not separately entail e then there is a choice of whether to remove a or b or both; but that choice will be arbitrary. This is not the only problem. If K is deductively closed and e is simply deleted from K then all the consequences of e

by itself and jointly with the remainder of K will remain. This means that *e* will almost certainly be very probable relative to $K-\{e\}$, which may not be as serious as assigning *e* invariably probability one, but is not much less so.

I think that it is possible to answer these objections, and the answer is twofold. It is, firstly, to drop the insistence on the deductive closure of K, or indeed of any body of background information, and secondly, to regard K as, in effect, an independent axiomatization of background information and $K-\{e\}$ as the simple set-theoretic subtraction of *e* from K. The requirement of deductive closure is quite unnecessary anyway within a Bayesian theory, since the probability function P_A relativized to any body A of information will necessarily assign the value 1 to any deductive consequence of the sentences in A whether or not that consequence itself is explicitly included in A (note that P_A assigns 1 because the notion of tautology is widened in effect to include consequences of A, not because $P_A(\cdot)$ is regarded—it is not—as a conditional probability $P(\cdot/A)$). Moreover, one's representation of one's background information is never a deductively closed set, and is typically a set of logically discrete statements, insofar as it is articulated at all. So modelling background information in this way, while an idealization, is not outrageously unverisimilar.

Suppose, though, that *e* is not an isolated part of background information: in other words it does not appear explicitly as a member of K where K is now characterized in the way I suggest. Suppose, for example, with Chihara, that *e* is nevertheless a strict consequence of some of the sentences in K. It may even be the case that among these other sentences in K were theories initially contrived for the explanation of *e*, which eventually became so inductively grounded that they acquired the status of certainty. What then? Here there seems to be no determinate way of characterizing one's state of knowledge on the counterfactual assumption that one does not know *e*, since not knowing *e* plausibly entails—if one knows the logical implications between the sentences in K and *e*—not knowing at least one other item of information currently in K (I am investing the concept of knowledge here with only a very minimal content, incidentally: namely, that if you know that $\{a_1, \ldots, a_n\}$ implies *b*, and you know $a_1, \ldots, a_n$, then you know *b*). But that is all that can be said, and we must conclude that in such cases the probabilities in the support function are relativized to an indeterminate state of background information, with the consequence that the support *e* gives to any newly advanced hypothesis is also indeterminate. However, the sorts of cases which are brought up in the literature tend to be those in which the evidence, like the statement describing the magnitude of the observed annual advance of Mercury's perihelion, is a logically isolated component of background information; and here relativizing to the set-theoretical subtraction of *e* from an—in effect—axiomatized K is well-defined.

But there appears to be a very powerful and familiar objection to regarding K as an axiomatization of background knowledge. This is that a deductive theory can be axiomatized in different ways, and the effect of removing one and the same axiom from two equivalent axiomatizations may well give rise to different consequence classes *i.e.* to non-equivalent theories. A simple example of this is the pair $\{a, b\}$ and $\{a, a \rightarrow b\}$ where a and b are logically independent sentences. Clearly, a and $a \rightarrow b$ are also independent and the axiom pairs generate the same sets of consequences, but if we remove a from both the resulting sets of consequences are distinct: b is in one and not the other, for example.

The representation-dependence of 'background minus *e*' should not, however, disturb us, disturbing though it may well appear at first sight. It looks disturbing only because we have inherited an ancient habit of thought that dies hard, namely that there is, somewhere in logical space, a purely objective logic of induction. There is not. There are sound planks that float amid the wreck of this beguiling idea, however; they constitute a theory of coherent degrees of belief, measured by a probability function, and a theory of rather weakly constrained inference based upon the properties of a *conditional* probability function. The resulting theory of belief-cum-inference is called Personalistic or Subjective Bayesianism. Now it is entirely consistent with this theory that different individuals may structure their knowledge in different ways. And if they do, it is only to be expected that the effect of selectively deleting items from this stock will vary from individual to individual. In that case, people's estimates of the confirmatory effect of old evidence may in principle vary (though shared educational experience means that it probably would not).

Let us now address Chihara's second objection to relativizing probabilities to $K-\{e\}$ is that while *e* may be logically isolated in this way, it may not be probabilistically isolated from the remainder of K ([1987] p. 553). Chihara points out that there may be things in K that are there because of the same observations that engendered *e*, and that these may make *e* itself highly probable: *e.g.* that the highly competent researcher A believes *e* may be a consequence of those observations; and this last statement makes *e* itself probable. But it is difficult to see why Chihara regards this as an objection. If *e* is still very probable relative to $K-\{e\}$, then all this means is that it will not count as a strong confirmation of *h*. There is nothing problematic about this: on the contrary.

4 HOW THE ANSWER SOLVES A PROBLEM

The extant objections to relativizing probabilites to $K-\{e\}$ in computing supports do not appear to be very difficult to answer. As we have seen, such a strategy is natural enough when we analyse incremental support in terms of the increase in credibility of *h* which *would now* be contributed by a knowledge of e. Moreover, the relativization to $K-\{e\}$ does seem to be essentially involved if we are to make sense of our intuitive evaluations of support. A rather striking example, over and above examples of the Mercury perihelion sort, is afforded by the following situation envisaged recently by Maher ([1988]), and further discussed in Howson ([1989b]) and Howson and Franklin ([1990]). We are asked to consider two possible states of affairs:

(i) an individual predicts the outcomes of the next 100 tosses of a coin. The coin is tossed 99 times, and the outcomes of these tosses are as predicted. This fact gives us a very high degree of confidence in the prediction of the outcome of the 100th toss;

(ii) an individual predicts the same outcome of the 100th toss as in (i), but now *after* having learned the outcomes of the first 99 tosses. Let us suppose that roughly sixty per cent of the outcomes of the 99 tosses in both this case and in (i) are heads, and that the predicted outcome of the 100th toss is of a head. Our intuitive degree of confidence in this prediction of the 100th toss, measured by the probability we assign that outcome in the light of all the data, is here probably around sixty per cent, whereas in (i) it is practically one.

Now for a Bayesian analysis of these judgments of support. First, some abbreviations. Let h be the hypothesis that a head will occur at the 100th toss of the coin, and let e be the description of the outcomes of the 99 tosses. A hypothesis whose consideration is clearly crucial for understanding our intuitive verdicts is that the subject has prior access, by some unspecified means, to reliable information about the outcomes of the 100 tosses. In (i) this hypothesis, call it m, is well supported by e, and in (ii) it is not. That is really the nub of the issue, and we shall now see that the relativization to $K-\{e\}$ is essential to its explanation.

By the probability calculus

$$P(h/e) = P(h/m\&e)P(m/e) + P(h/-m\&e)P(-m/e) \qquad (2)$$

In both (i) and (ii) $P(h/m\&e)$ is equal to 1, since we know that the subject predicted h, and therefore m implies that h is true. Hence in (i) and (ii)

$$P(h/e) = P(m/e) + P(h/-m\&e)P(-m/e) \qquad (3)$$

Now $P(m/e) = P(e/m)P(m)$ divided by $P(e)$. Let us now consider case (i). Here $P(e/m) = 1$, again since it is background information that the subject predicted e as well as h (though it is not in background information that e is true), and so m implies e modulo this information. Moreover,

$$P(e) = P(e/m)P(m) + P(e/-m)P(-m),$$

and $P(e/m) = 1$ while $P(e/-m)$ is going to be very small indeed: the latter quantity is really just the probability of predicting correctly 99 tosses of a coin by chance. Hence P(e) is approximately equal to P(m) and so P(m/e) is close to 1; therefore so too is P(h/e).

Now for (ii). What is P(m/e) here? Well, the background information in this case now includes the information that e is true, and also that the subject predicted h after being acquainted with e's truth. Let us look, as in (i), at P(e/m) and P(e), where, in the light of the discussion in the Section 3, we are relativizing the probabilities to background minus e. Relative to this truncated background, there is no more reason to believe in the truth of e given m than otherwise, and so we can plausibly (*i.e.* following the reasoning processes of *l'homme moyen sensuel*, whom in this context we can take to possess an averagely unbiased outlook on this sort of problem at least) set P(e/m) equal to P(e). Hence P(m/e) = P(m), which, we can assume, is extremely small. Hence we can equate P(h/e) approximately to P($h/-m\&e$). Taking $-m$ as before to be equivalent to the null hypothesis of a purely chance agreement between prediction and outcome, we obtain, by the usual Bayesian reasoning, a value for P(h/e) reflecting (approximately) the proportion of heads in e.

So we explain our intuitive verdicts by relativizing to K $-\{e\}$ in both cases, though in (i) e was not in K and so the relativization made no difference. At this point it might be objected that we could more easily have explained the difference in support for h in (i) and (ii) by setting P(e/h) = P(e) = 1 in (ii), in line with Glymour's prescription. But the objection cannot be sustained, because it is easy to see that were we to follow that strategy we should ignore the constitution of the sample e itself in (ii), and yet, as we saw, the measured, frequency of heads in that sample is, after all, the sole determinant of our confidence in h. No; the only correct way of explaining our intuitive verdict in (ii), as in the case of Mercury's perihelion, is to relativize to K $-\{e\}$. Lest there be any lingering doubt that Glymour's thesis, that P(e) in the support computation must be set equal to 1 when you learn e's truth, is wrong, consider the classical Bayesian analysis of the hypothesis h' that a coin has a given propensity to yield heads, proposed after noting the coin's mass-distribution. The coin is then tossed n times, and the outcomes described by e'. Does the fact that the experiment delivering e' has been performed mean that P(e') is to be set equal to 1 when P(h'/e') is computed? Of course not; any Bayesian will point out that this is a misuse of the formula.

5 GARBER'S ANSWER

It remains finally to review a quite different attempt to solve the old evidence problem, due to Garber ([1983]), Niiniluoto ([1983]), Jeffrey ([1983]) and others. I shall limit myself to a brief description of Garber's account, though the idea common to all three (and which was first suggested by Glymour) is that it is not the knowledge of the old e which increases the probability of h above its initial level, but learning *that* h *explains* e. For example, Einstein discovered only after writing down the equations of General Relativity that they entailed the anomalous perihelion advance of Mercury. In order to accommodate this sort of logical learning Garber proposes, firstly, extending some initial domain of P to include all statements of the form $h \vdash e$ for all h, e in the initial domain and closing off again under truth functional composition (the entailment sign is treated as a primitive), and secondly, amending the probability axioms by assigning probability 1 only to those *truth-functionally valid* combinations of sentences in the enlarged domain, and adding as a further axiom a 'modus ponens' condition

$$P(a\&b\&(a \vdash b)) = P(a\&(a \vdash b)).$$

The result system models a type of logical as well as empirical learning, at any rate to the extent that it can be proved that there is an infinite number of probability functions P with the property that there are sentences h and e such that $P(e)=1$, $0<P(h \vdash e)<1$, $0<P(h)<1$, and $P(h/h \vdash e>P(h)$.

It does not, of course, follow from this result that any particular case, like the Mercury perihelion one, will exemplify such 'logical learning'; indeed, it is currently far from clear that Garber's analysis can be made to fit all or any such cases. Also, advocates of this whole approach accept uncritically what is has been the burden of the earlier part of this paper to deny, that e's being known must be reflected in its being assigned probability 1. But whatever the virtues and vices of Garber's construction (and it has stimulated a good deal of discussion), *it does not solve the old evidence problem*: it is not, I am now going to argue, the learning merely of an entailment which is responsible for the increase in confirmation when we judge that old evidence supports a new theory. In the first place, we should note that the Garber system is not as it stands valid for more general forms of explanation, like statistical explanation, than entailment modulo satisfaction of initial conditions, yet there seems no reason to suppose that learning of these more general explanatory relations should not exercise the same confirmatory effect on this account. But the logic of explanation in general, unlike that of deduction, is notoriously ill-understood, and incorporating criteria of explanation into the Bayesian system, at any level, is something to be undertaken only, I should have thought, as a very last resort. Secondly, and more important, it is not difficult to think of examples of old evidence e and hypotheses h such that it was already *known* that h explains e, and yet in which e is nevertheless considered to have supported h. Newton was perfectly well aware that Kepler's Laws were explained by his theory when he constructed it, since that this should be so was of course an explicit constraint on the construction of that theory; yet Newton and practically everybody else regarded the inverse square law together with the laws of motion to be supported by Kepler's Laws (Franklin and Howson [1985]). And Earman, in his systematic discussion ([1989]) of the old evidence problem, has pointed out that while Einstein himself may have discovered that his equations entailed the observed value of Mercury's perihelion advance, we still

> 'want to say that the perihelion phenomenon was and is good evidence for Einstein's theory. But along with most students of general relativity, the first thing we may have learned about the theory, even before hearing any details of the theory itself, was that it explains the perihelion advance.' (p. 16.)

Much more homely counterexamples to Garber *et al.*'s thesis can easily be constructed. For example, suppose a coin is tossed n times and the observed relative frequency of heads is r/n. The hypothesis that the coin has a propensity $(r/n) \pm f(n)$ to fall heads is then proposed. According to both statistical practice and theory such a hypothesis is well-supported by the data (in combination with suitable background information), even though it was put forward expressly as the best explanation of that data. The sample-average statistic r/n is what is called a maximum-likelihood estimator of the propensity, and maximum-likelihood estimation is no less than a general algorithm for generating best explanatory hypotheses from the data.

6 CONCLUSION: ACCOMMODATION AND PREDICTION

We have, not entirely unwittingly, strayed into another highly controversial debate, about whether hypotheses deliberately constructed to explain data are thereby at all supported by those data. I have recently argued at length (Howson [1989a], [1989b], and Howson and Urbach [1989] Chapter 11) that hypotheses may in suitable circumstances—which I set out—be confirmed by the data they were constructed to explain, and I have also shown, I believe, where the usual arguments that this is not possible break down. So I hope I shall be permitted to refer to the reader to those sources and not argue any further here for this position. But if, as I hope, it is granted, then it indicates that Garber *et al.*'s diagnosis of why old evidence confirms is incorrect, and—fortunately—we don't have to tamper with classical Bayesianism in the way they recommend.

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REVIEW ARTICLE

Justification in the Natural Sciences

PETER GÄRDENFORS [1988]: *Knowledge in Flux*. MIT Press, *xi*+262 pp.

RONALD GIERE [1988]: *Explaining Science*. University of Chicago Press, *xxi*+321 pp.

ADOLF GRÜNBAUM and WESLEY SALMON (*eds.*) [1988]: *The Limitations of Deductivism*. University of California Press, *xxv*+168 pp.

PAUL THAGARD [1988]: *Computational Philosophy of Science*. MIT Press, *xiv*+240 pp.

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1 *Some Meta-Epistemological Matters*
2 *Two Epistemological Extremes*
3 *Epistemology in Flux*
4 *Naturalizing Philosophy of Science*
5 *Computation and Epistemology*
6 *Deductivism and Epistemology*
7 *Conclusion*

Philosophy of science includes the epistemology of natural science as a major component. The epistemology of natural science seeks a correct explanation of the conditions for scientific knowledge of the natural world. A central part of such epistemology is the theory of scientifically *justified* belief. A scientifically justified belief, roughly characterized, is a belief appropriately warranted to be a component of scientific knowledge. The conditions for a belief's being thus appropriately warranted attract much controversy among epistemologists of natural science.

These days philosophers of science are nowhere near consensus on a substantive epistemology of science. And this seems to be in keeping with the turbulent history of matters philosophical. One wonders whether we can find any common ground from which to measure epistemological progress.

I SOME META-EPISTEMOLOGICAL MATTERS

Perhaps we can make some epistemological progress by attending to the *meta*-epistemology of natural science. Such meta-epistemology focuses on the goals and presuppositions of an epistemology of natural science. Let's examine some meta-epistemological issues about scientific *justification* in particular. A comprehensive meta-epistemology would need to answer numerous issues about scientific truth and belief, if we assume that scientific knowledge has a truth condition and a belief condition. But questions about scientific justification alone will be enough for this paper.

Philosophers of science are often unclear on the main goals of a theory of scientific justification. We can usefully distinguish three main goals:

(a) the *semantic* project of specifying, in informative terms, what it means to say that a theory or hypothesis is scientifically justified.
(b) the *explanatory* project of identifying informative conditions that state when, or in virtue of what, a theory or hypothesis is scientifically justified.
(c) the *evaluative* project of formulating standards for evaluating whether a particular theory or hypothesis is scientifically justified.

A proper understanding of what constitutes success for these projects would contribute much to the formulation of a theory of scientific justification. Philosophers often propose solutions to these projects without clarifying what makes, or at least would make, their solutions correct. This, however, is not conducive to genuine progress in epistemology.

The semantic project aims to define the term 'scientific justification' by informative synonymous terms. The explanatory project aims to identify informative truth-conditions for the schema 'Theory or hypothesis *H* is scientifically justified for person *S*'. These truth-conditions will be explanatory conditions that ideally are individually necessary and jointly sufficient for *H*'s satisfying that schema. The evaluative project aims to formulate criteria for deciding whether a theory or hypothesis is scientifically justified. It thus aims to go beyond definition and explanation, to a *method for discerning* what theories or hypotheses are scientifically justified. We should not conceive of the evaluative project as simply the application of the results of the explanatory project. The explanatory project might involve complex causal considerations that preclude its being applicable for evaluation.

In any case, an adequate meta-theory for scientific justification needs to answer such questions as these:

(a′) *Question for the semantic project*: What does it mean to say that a definition of 'scientific justification' is itself justified?
(b′) *Question for the explanatory project*: In virtue of what can explanatory

conditions be justified as an answer to the question of what constitutes scientific justification?

(c′) *Question for the evaluative project*: By what standards can we justifiedly evaluate certain standards as being adequate for discerning scientifically justified beliefs (*i.e.*, as providing a correct solution to the evaluative project)?

The failure to answer such questions about the explanatory and evaluative projects results in *meta-epistemic naiveté* of a philosophically objectionable sort. Such naiveté comes from one's lacking a cogent reply to a serious challenge to the justification of one's standards for scientific justification.

If, however, one seeks an answer to the questions about the explanatory and evaluative projects, another problem threatens. If one already accepts certain explanatory conditions and evaluative standards as answers to projects (b) and (c) above, one will naturally invoke these conditions and standards to answer questions (b′) and (c′). This would not be surprising, but what would it really accomplish? Since circular argument lacks rational cogency in general, we should avoid it in our effort to support our standards for scientific justification. But how can we? This is an urgent question for any epistemology of natural science.

I shall ask how the books under review bear on the aforementioned epistemological projects and questions. A contrast between two general approaches will set the stage for this question.

2 TWO EPISTEMOLOGICAL EXTREMES

At one end of the epistemological spectrum we have *scientific-community authoritarianism* of the sort familiar from Thomas Kuhn's [1970a] account of theory choice. At the other end we have *epistemological-standard authoritarianism* stating that the standards (*e.g.*, evaluative norms and explanatory principles) for scientific justification are epistemically independent of the actual decisions and practices of scientists. The latter view typically finds an epistemic basis for epistemological standards either in certain metaphysical principles (as in certain Aristotelian philosophies of science) or in certain principles of inductive or deductive logic (as in Carnap's [1950] confirmation theory). Between the two extremes we have a number of views acknowledging the epistemological significance both of actual scientific decisions and practices and of certain standards independent of such decisions and practices. But let's not leave the extremes yet.

Scientific-community authoritarianism states that the decisions and practices of a scientific community provide the ultimate standard for settling epistemological issues. Kuhn endorses this view regarding paradigm choice as follows: 'As in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community' ([1970a], p. 94; *cf.* p. 170). Kuhn also endorses such a view regarding scientific theory choice in general. He holds that the best way to decide whether one scientific theory is more reasonable than another is as follows:

> Take a *group* of the ablest people with the most appropriate motivation; train them in some science and in the specialties relevant to the choice at hand; imbue them with the value system, the ideology, current in their discipline (and to a great extent in other scientific fields as well); and, finally, let them make the choice ([1970b], p. 237).

Kuhn's approach to scientific justification is thus essentially social. It explains the justification of scientific theories not by ahistorical epistemological standards, but by the preferences of a scientific community, *i.e.*, its shared values, likes, and dislikes.

Kuhn gives no reason to think that the values in question have epistemological significance independently of the preferences of a scientific community. Kuhn apparently holds that a scientific community's preferences and practices, rather than independent scientific values, are epistemologically fundamental. This approach leads to the importance of historical case-studies in the philosophy of science. The way to explain scientific justification, on this approach, is to explain the actual preferences and practices of a scientific community.

Kuhn's approach contrasts sharply with Carnap's epistemological-standard authoritarianism. Carnap's account of scientific justification relies on his account of logical probability. Carnap regards logical probability as similar to logical implication, as a matter of 'partial implication' ([1966], p. 32). Logical analysis, on Carnap's view, provides us with our understanding of relations of logical probability. Thus: 'By a logical analysis of a stated hypothesis *h* and stated evidence *e*, we conclude that *h* is not logically implied but is, so to speak, partially implied by *e* to the degree of so-and-so much ([1966], p. 33).

Carnap holds that his concept of logical probability is especially important for metaepistemic scientific statements, *i.e.*, statements about justification in the sciences. He explains:

> We say to a scientist, 'You tell me that I can rely on this law in making a certain prediction. How well established [justified?] is the law? How trustworthy [justified?] is the prediction?' . . . I believe that, once inductive logic is sufficiently developed, he could reply, 'This hypothesis is confirmed to degree .8 on the basis of the available evidence.' A scientist who answers in this way is making a statement about a logical relation between the evidence and the hypothesis in question. . . . His statement that the value of this probability is .8 is, in this context, not a synthetic (empirical) statement, but an analytic one. It is analytic because no empirical investigation is demanded ([1966], p. 35).

Carnap also claims that analytic statements of logical probability can be known a priori ([1966], p. 36). So on his view questions regarding the acceptability of a theory relative to certain evidence are answerable apart from any examination of the preferences or practices of actual scientists. Indeed, such questions are answerable apart from *any* empirical investigation. Here we have one key point of contrast with Kuhn's scientific-community authoritarianism.

Another key point of contrast comes from Carnap's account of the *evidence* relative to which theories can be logically probable. Scientific evidence, on Carnap's account, comes from 'observations' that are either passive and nonexperimental or active and experimental ([1966], pp. 6, 40). Scientific observations are of 'single facts', and thus give observers access to what needs scientific explanation. The evidential status of observations, on Carnap's account, owes nothing to the preferences or values of a scientific community.

Carnap's observational standard for evidence and probabilistic standard for theory-support are not inferred from actual scientific beliefs and practices. Carnap thus does not examine the history of science as a basis for recommending his epistemological standards. Instead, he recommends his standards on a priori grounds, specifically on nonempirical grounds concerning what is required for empirical knowledge. Carnap begins his epistemology of science with a certain a priori notion of empirical knowledge, and then uses this notion to formulate standards concerning the justifiability of knowledge claims made by scientists. These standards, rather than the views and practices of actual scientists, have epistemological authority for Carnap.

We have, then, two epistemological extremes: Kuhn's empirical approach that relies on an examination of actual scientific practices to formulate and to justify standards for scientific justification, and Carnap's a priori approach that relies on an a priori notion of empirical knowledge to formulate and to justify standards for scientific justification. Given only these extremes, we face a troublesome dilemma. Either (a) given Kuhn's empirical approach, we preclude noncircular epistemically critical assessment of actual scientific views and practices, or (b) given Carnap's a priori approach, we need give no independent epistemic significance to the fundamental theoretical goals and methodological assumptions of a scientific community.

Option (a) robs epistemology of its normative clout, at least with respect to certain scientific views and practices. If actual scientific views and practices are the ultimate basis for testing epistemological principles, we will have no independent epistemological basis from which we can critically assess those views and practices. If there were such an independent basis, the views and practices in question would not really be the ultimate basis for testing after all. It will not help here to suggest that certain scientific views and practices can be epistemically assessed on the basis of *other* scientific views and practices. For this suggestion goes nowhere without an account of why the latter views and practices, rather than conflicting views and practices, have epistemological authority. Such writers as Toulmin [1972] and Laudan [1977] have illustrated that the history of scientific views and practices is sufficiently diversified to make any unqualified epistemological appeal to such views and practices highly problematic.

Option (b) risks divorcing our epistemology from the actual natural sciences. If our standards for scientific justification can justifiably come solely from an a priori notion of knowledge that is unconstrained by the goals and assumptions of actual scientific practice, we shall be hard put to explain why these standards are *scientifically* relevant. The problem is that purely a priori standards for scientific justification need have no epistemic basis whatsoever in what is theoretically appropriate for accomplishing the cognitive goals of actual scientists.

We could easily formulate a priori standards for scientific justification that either have no direct relevance to the cognitive goals of actual scientists or imply that all past and present scientific goals and assumptions are not scientifically justifiable. So long as these a priori standards need not be constrained by the cognitive goals of actual scientists, but need only come from one's preferred notion of empirical knowledge, we could have a wide-ranging plethora of such standards that relate at best coincidentally to the cognitive goals of actual scientists. A priori standards for justification are not hard to come by, if they can come, as Carnap suggests, solely from one's preferred notion of empirical knowledge. It seems, however, that our standards for *scientific* justification must have some substantive epistemic connection to actual cognitive goals of scientists; otherwise, these standards will fail to be scientific in any relevant sense. Carnap's a priori approach does nothing to guarantee this needed substantive connection.

Any adequate epistemology of natural science must avoid the aforementioned dilemma raised by the epistemological extremes of Kuhn and Carnap. Einstein ([1949], p. 684) apparently had something like this dilemma in mind when he noted that 'epistemology without contact with science becomes an empty scheme [and] science without epistemology is . . . primitive and muddled'. Let's ask whether the books under review make any contribution to avoiding the dilemmas I have outlined. We shall see that meta-epistemology deserves much more attention than it has received in recent philosophy of science.

3 EPISTEMOLOGY IN FLUX

Gärdenfors [1988] develops a theory of rational changes of belief that bears on belief-expansion, belief-contraction, and belief-replacement. One of his main goals is to formulate 'rationality criteria' for belief-changes. Gärdenfors takes rationality criteria to be important for 'evaluating' the components of an epistemological theory. He illustrates (p. 8):

> For example, if sets of sentences are used as models of epistemic states [*i.e.*, belief-states], then *consistency* is a basic rationality requirement on such a set. Or, when evaluating changes of belief, we require that the change be the *minimal* one needed to accommodate the epistemic input that generates the change (*cf.* pp. 9, 22, 49).

Here we have two of Gärdenfors's main rationality requirements. But Gärdenfors is unclear about what justifies his rationality requirements. Perhaps they result from a stipulative definition of 'rationality'. But if so, why should we think that they bear at all on rational belief-change in the natural sciences? Perhaps we shouldn't.

In any case, various writers have contested rationality requirements of the sort Gärdenfors proposes. For instance, Nicholas Rescher ([1979a], pp. 165–78), ([1979b], pp. 127–41), ([1988], pp. 73–83) has challenged the view that inconsistency of belief, including scientific belief, is automatically irrational. In the same vein, Eugene P. Wigner [1960] has suggested a situation where inconsistency of one sort could be justifiable in theoretical physics:

> We now have, in physics, two theories of great power and interest: the theory of quantum phenomena and the theory of relativity. These two theories have their roots in mutually exclusive groups of phenomena. Relativity theory applies to macroscopic bodies, such as stars. The event of coincidence, that is in ultimate analysis of collision, is the primitive event in the theory of relativity and defines a point in space-time, or at least would define a point if the colliding particles were infinitely small. Quantum theory has its roots in the microscopic world and, from its point of view, the event of coincidence, or of collision, even if it takes place between particles of no spatial extent, is not primitive and not at all sharply isolated in space-time. . . . So far, the two theories could not be united. . . . All physicists believe that a union of the two theories is inherently possible and that we shall find it. Nevertheless, it is possible also to imagine that no union of the two theories can be found (pp. 11–12).

If we suppose that general relativity requires point-events in space-time, whereas quantum theory excludes such point-events, we can easily imagine how our physics might rationally exhibit inconsistency of a sort. Such inconsistency could, it seems, be justifiable relative to conflicting data needing explanation. At least, Gärdenfors does nothing to show that such inconsistency is automatically irrational. This is a serious shortcoming in the defense of his rationality criteria.

Gärdenfors's view of the rationality of *minimal* belief-change is similarly uncompelling. The only argument Gärdenfors gives for his view here is: '[I]nformation is in general not gratuitous, and unnecessary losses of information are therefore to be avoided' (p. 49). For this line of argument to succeed, Gärdenfors must assume that one's antecedent beliefs automatically have a degree of rationality that makes their being altered or relinquished irrational in the absence of specific underminers. But why should we think that antecedent beliefs automatically have such rationality? Although Gärdenfors does not answer, one will think this only if one endorses some version of *epistemic conservatism*, the view that a belief is made probable to some extent for a person simply by that person's holding the belief. But epistemic conservatism is problematic. It runs afoul of the apparent fact that one can hold a belief for which justifying evidence is altogether lacking. The mere holding of a belief seems not to generate evidence on its own. Gärdenfors does nothing to explain away this problem for his epistemic conservatism. (For elaboration on related problems facing epistemic conservatism, see Moser ([1985], Chapters 2, 3), ([1989], pp. 63–9, 183–6), and Foley ([1987], Chapter 7).

Gärdenfors also endorses the following rationality criterion: 'Logical consequences of what is accepted should also be accepted' (p. 22). For any proposition, *P*, the proposition that ($P \vee Q$) is, of course, a logical consequence of *P*. So if, on Gärdenfors's criterion, I accept that quantum theory is correct, then I rationally should also accept (a) that either quantum theory is correct or Chicago contains just one blade of grass, (b) that either quantum theory is correct or Chicago contains just two blades of grass, and so on *ad infinitum*. Each member of this endless series of consequences is a semantically distinct proposition, and thus involves a distinct event of acceptance. (Gärdenfors (p. 23) apparently does think of acceptance as requiring a psychological event of 'treating' a proposition as true.) Evidently a person could satisfy Gärdenfors's rationality criteria concerning consistency, economy, and implication by expanding beliefs in accord with the trivial series just illustrated.

But why should we think that rationality requires us to accept all the trivial consequences of what we accept? Our acceptance-sets are, after all, limited at least because of time constraints. Acceptance takes times, and we simply do not have the time to accept all the logical consequences of what we accept. Given our cognitive goals, some logical consequences are important whereas others are trivial; and rationality seems to require that we avoid the trivial to leave room for the important. It is quite unclear how Gärdenfors's rationality criteria allow for this consideration.

Gärdenfors holds that his epistemological theory is nonempirical in one sense: it does not presume any account of an external world beyond one's belief-states. Thus he claims that '. . . the rationality criteria governing epistemic dynamics [*i.e.*, belief-change] can be, and should be, formulated independently of the factual connections between the epistemic inputs and the outer world' (p. 9). Such a strategy could be important if one aimed to avoid begging questions against skepticism regarding knowledge of the external world. But what then is supposed to justify Gärdenfors's rationality criteria? Clearly he cannot appeal to features of the cognitive goals of actual scientists, as such features are, at least from our standpoint, features of the external world. Perhaps, then, Gärdenfors's criteria are supposed to have an a priori basis.

Gärdenfors does suggest that rational epistemic states are 'idealizations' of human cognitive states (pp. 9, 10). He regards idealized epistemic states as 'equilibrium states', since such states must reach an equilibrium that satisfies his rationality criteria.

But we need an answer to this question: what degree of idealization is appropriate for rational assessment of belief-change, including belief-change in the natural sciences? Idealization comes in numerous degrees and forms; it is limited only by one's ability to imagine a standpoint from which assessments can be made. But surely not all forms of idealization are appropriate for assessing the rationality of belief-change, especially belief-change in the natural sciences. We need to explain, then, what makes an idealized epistemological theory pertinent to scientific justification. This is just a reformulation of the problem attributed above to Carnap's a priori epistemological approach. This problem threatens Gärdenfors's epistemological approach too, but he gives no hint how to avoid it. Thus Gärdenfors fails to escape the dilemma raised in Section 2 above. The problems facing Gärdenfors's account stem from inadequate consideration of the meta-epistemological issues raised above in Section 1.

4 NATURALIZING PHILOSOPHY OF SCIENCE

Giere [1988] has developed a naturalistic account of scientific rationality. It requires only the effective use of available means to achieve desired scientific goals. Giere does not concern himself with showing how claims of scientific knowledge can be justified in a noncircular manner that would satisfy skeptics or even traditional epistemologists. Such a project of justification, according to Giere, should be regarded as 'scientific apologetics', and should be contrasted with his 'naturalized philosophy of science'. Giere aims at a 'cognitive theory' of science that explains science itself in the way that scientific theories explain certain natural phenomena. His theory is 'cognitive' in that it draws on the explanatory resources of the cognitive sciences.

Giere seems to be sensitive to a dilemma similar to that raised in Section 2 above. He notes (p. 2) that philosophers typically employ a priori ideals, and minimize actual social context, in assessing the natural sciences, whereas sociologists of science emphasize actual social factors of science above all else. Giere faults typical philosophical approaches to scientific rationality for failing to 'demonstrate that a particular principle captures a relationship that is uniquely [scientifically] rational' (p. 3). He also questions whether these approaches can give an adequate account of the widespread methodological disagreement among scientists, *i.e.*, an account that does not imply that most scientists are irrational. Giere faults the sociological approach to science for failing to explain 'the obvious success of science'. The success of science, according to Giere, does not result solely from changing social relationships. It results from causal interaction between scientists and *the world* (p. 4).

Giere's naturalized theory of science thus endorses realism. It aims to explain how scientists use their cognitive capacities (*e.g.*, perception, memory, language) for interacting with the world. Scientific theories, on Giere's account, are 'representations', *i.e.*, 'internal maps of the external world' (p. 6). Giere characterizes such internal maps as 'theoretical models' of the world; thus:

> Theoretical models are the means by which scientists represent the world—both to themselves and for others. They are used to represent the diverse systems found in the real world: springs and pendulums, projectiles and planets, violin strings and drum heads (p. 80).

We have, then, a sharp distinction between *theoretical models* and *real systems*, the former being representations of the latter. Giere calls his view *constructive realism*, since it takes theories to assert a similarity between some part of a representational model and a system in the real world (p. 97).

Giere's constructive realism raises some controversial issues, one of which is especially noteworthy. What exactly is the sort of similarity that is supposed to hold between a theory and the world? Giere denies that he needs a notion of approximate truth (p. 106). Further, he wants a notion of similarity that allows for respects as well as degrees of similarity (pp. 81, 107). Now, anything, it seems, is similar in some way or another to anything else; so we need some limit on *relevant* respects of similarity. But Giere claims that scientific 'models themselves provide an upper limit on the respects in which similarity can be claimed' (p. 93). He also holds that which aspects of a model are claimed to be similar to the world remains to be resolved on a case-by-case basis by scientists themselves (p. 97).

Even so, Giere needs to give some specificity, some semantic definiteness, to his general notion of similarity. As it stands, this notion functions as an empty place-holder. Perhaps models do place an upper limit on respects of similarity. It nonetheless is Giere's job, given his theory of science, to explain what is common to all relevant cases of similarity, or at least what exactly constitutes the similarity relation between scientific models and the world. A truth-relation, such as that characterized by Mackie [1973] and by myself [1989], would probably work, but Giere wants to avoid any specific semantic notion here. I suspect he needs a semantic notion, such as a notion of truth, if his constructive realism is to be sufficiently precise to be informative. Otherwise, we are left with unqualified talk of similarity, and such talk is too ambiguous to do informative theoretical work. (For further relevant discussion, see Putnam ([1981], Chapter 3.)

Let's turn now to the question of how Giere would justify the explanatory standards that make up his constructive realism and his naturalistic account of scientific rationality. Giere's naturalism affirms that 'theories come to be accepted through a natural process involving both individual judgment and social interaction'; on this view, 'no appeal to supposed rational principles of theory choice is involved' (p. 7). Giere applies such naturalism to his account of scientific explanation:

> A theory of explanation, for me, is not to be judged by philosophical standards, but by the standards of the cognitive sciences. That is, an empirical theory of explaining would be judged by the sorts of evidence relevant to theories of other higher level cognitive activities such as language comprehension and problem solving (p. 105).

In the same vein, Giere proposes that we can avoid the problem of relativism concerning standards for rational theory-choice by focusing simply on the cognitive processes (*e.g.*, those involved in representation and judgment) shared by all scientists (p. 37).

Giere thus proposes that we replace all talk of rationality with talk of 'effective goal-directed action' (p. 9). He denies that there is a coherent notion of categorical rationality according to which there can be rationality of ultimate goals as well as means. But Giere still wants to leave room for normative claims about how science should be pursued: 'What a naturalistic approach requires is that science policy be based on solid empirical findings about effective strategies for pursuing various scientific goals' (p. 10). On this approach, scientific rationality is a system of hypothetical imperatives concerning effective goal-directed action.

Giere's doubt about the relevance of a notion of categorical rationality seems plausible, especially since such a notion need have no direct bearing on the cognitive goals of actual scientists. But Giere's use of the cognitive sciences to delimit the vocabulary and the relevant issues for a philosophy of science raises troublesome questions. One obvious question is: What justifies Giere's using the cognitive sciences instead of, say, neuroscience or social science? Giere finds his answer in evolutionary theory:

> By looking back at evolutionary history, scientists themselves can better understand their own cognitive situation and investigate the development of their own cognitive capacities. . . . Using our evolved cognitive capacities, we extend our knowledge of the world, including our knowledge of our own cognitive abilities. . . . There is no point in seeking to characterize the ideally rational scientist. Rather, we should seek to explain the evolution of science in terms of the selective mechanisms operating on natural variations among real scientists (pp. 14–15).

On this approach, we take evolutionary biology as our epistemological basis, from which we isolate certain cognitive capacities as having evolved along with human bodies. The cognitive sciences study those capacities, but we do not, on Giere's approach, raise epistemological questions about the evolutionary biology from which those capacities were isolated. Evolutionary biology is where Giere's epistemological questions stop.

Giere thus remains content with the observation that 'the general *reliability* of the mechanisms that generate many of our perceptual and cognitive judgments is, from an evolutionary perspective, not open to serious question' (p. 13). Even if this observation is correct, we can still ask whether there is an unanswered *philosophical* question about the reliability of our cognitive mechanisms. Suppose that the reliability of cognitive mechanisms is just the truth-conduciveness of those mechanisms, *i.e.*, their tending to give us truth more often than falsehood. Even if evolutionary biology were to assume such reliability of our cognitive mechanisms, we could still ask, as philosophers, whether this assumption is correct. And this question would seem to make good sense, even though it resists any easy answer. It is doubtful, for instance, that there is any necessary connection between what has evolutionary success and what is reliable in the sense indicated. (See Stich [1985] and Feldman [1988] for relevant discussion.)

The key point, however, is that Giere's use of evolutionary biology results in a philosophically shallow treatment of questions about the reliability of our cognitive mechanisms. An epistemologically unexamined evolutionary biology is not worth having, at least from a philosophical standpoint. If we allow the assumptions of current evolutionary biologists to delimit and to settle our epistemological questions, we are at best guilty of a naive realism about evolutionary biology. And naive realism here is no better, philosophically, than naive realism elsewhere. Even if the main assumptions of evolutionary biology are largely correct, we as philosophers need to formulate a tenable epistemology that enables us to explain and to defend that this is so. More generally, any naturalizing of philosophy of science must rest on an epistemology that justifies the naturalizing in the way proposed. Otherwise, we have at best a philosophically arbitrary stopping-point, rather than an epistemologically sound foundation.

Further, even if we take evolutionary biology as our epistemological basis, we seem not to be led automatically to the cognitive sciences as our explanatory system for a philosophy of science. The cognitive sciences take a high-level, 'top-down' approach to psychological explanation that avails itself of a seemingly mentalistic, 'folk-psychological' vocabulary, *e.g.*, talk of beliefs, schemas, and representations. In contrast, eliminative materialists, such as Stich [1983] and Churchland [1986], endorse evolutionary biology but have doubts about the folk-psychological vocabulary of the cognitive sciences. They propose instead that psychological explanation should take a low-level, 'bottom-up' approach that, in the spirit of reductionism, aims to make do with the vocabulary of physics, chemistry, and neuroscience. Giere's folk-psychological model needs to be shown superior to competing neuroscientific models. Since Giere fails to show the superiority of his top-down approach to a bottom-up, neuroscientific approach, his case for a 'cognitive' theory of science remains seriously inconclusive.

5 COMPUTATION AND EPISTEMOLOGY

Thagard [1988] presents a 'computational' philosophy of science relying on some ingredients of contemporary cognitive psychology. His view of the mind postulates (a) mental structures or representations corresponding to data structures in a computer program and (b) mental processes that operate on mental representations in a way analogous to the role of algorithms, or well-defined procedures, in a computer program. Thagard provides a computational account of scientific knowledge that explains how such knowledge can be represented in computer programs. He thus devotes much space to describing his own computer program for problem-solving and induction. I shall be more concerned, however, with the epistemological assumptions of Thagard's computational account.

Thagard regards a scientific theory as a complex of data structures, as a computational system of concepts, rules, and problem solutions (pp. 34, 48). His approach to theory evaluation is rather straightforward: 'we want to accept a theory only if it provides the *best* explanation of the relevant evidence' (p. 75). But Thagard's notion of 'evidence' is unclear; he shifts without discrimination between talk of evidence and talk of 'facts' (p. 78). Evidence concerning the natural world can plausibly be understood as a probabilistic indicator of what is factual in the natural world, but it is odd to identify such evidence with facts in the natural world. Agreement about evidence, it seems, need not entail agreement about the facts allegedly indicated by that evidence.

Thagard argues that certain actual cases of scientific reasoning provide standards for evaluating explanatory theories, viz., consilience (comprehensiveness), simplicity, and analogy. Analogy in explanation occurs when we observe that objects A and B share certain properties, and learn that A's having those properties is explained by its having a further feature, and then infer that B's having those properties is also explained by this further feature. Thagard maintains that analogical problem-solving can increase a theory's explanatory power. The simplicity of a hypothesis, on Thagard's account, is determined by the number of additional 'cohypotheses' needed for the hypothesis to accomplish its explanations. If H has a lower ratio of cohypotheses to facts explained than does H', then H is simpler than H'. It is unclear, however, how Thagard would handle cases where the facts being explained differ in relative importance, or where *H* explains more *kinds* of facts but fewer individual facts than *H'* (*cf.* p. 91).

Consilience requires that a theory explain not only the most phenomena, but also the *most important* phenomena. Importance of a phenomenon, such as a rule, is a function of its usefulness in problem-solving as well as its probability (p. 89). But Thagard now owes us an account of relative importance of problem-solutions. For something can be useful in problem-solving while failing to contribute to the most important problems we face. If scientific justification is a function of a theory's usefulness in problem-solving, it would seem also to be a function of a theory's contributing to the solution of cognitively important problems. Cognitive importance of problems resists any easy explanation, but it demands explanation of some sort from a Thagard-style account of justification.

Thagard pursues the question of how we can justify the use of such a form of reasoning as inference to the best explanation. He argues that we can arrive at justifiable normative principles of reasoning by reflection on how everyday and scientific reasoning actually works. In short:

> [T]he justification of a set of normative principles is based, not on the reflective equilibrium of any individual or group, but on the place of the principles in a defensible inferential system. Defense is based on arguments that the system is coherent . . . (p. 130).

Thagard uses the following standards to assess the comparative coherence of inferential systems (p. 129):

> *Robustness*, or the extent to which the normative principles account for inductive practice.
> *Accommodation*, or the extent to which background theories account for deviations of inductive practices from the normative principles.
> *Efficacy*, or the extent to which, given background theories, the following of the normative principles promotes the satisfaction of relevant inferential goals.

Inferential systems that are maximally coherent by these standards, according to Thagard, are maximally defensible, and thereby lend justification to their constituent normative principles.

Thagard proposes a straightforward justification for his standards for coherence:

> I have advocated these criteria because they seem to be the ones actually used when we set out to evaluate inferential practices, and because they seem to promote the establishment of the sorts of inferential principles we want. In short, the criteria are, in an extended sense, robust and efficacious (p. 131).

Thagard denies that there is any vicious circularity here, and adds that he does not hold out for 'a full foundationalist justification' of inferential practice.

I do not know what Thagard means by 'full foundationalist justification';

but I suspect that his offered justification does fall into unconvincing circularity of the sort shunned in Section 1 above. If Thagard is not saying that his criteria for coherence are justified by considerations of coherence, specifically by considerations of robustness and efficacy, it is altogether unclear what he is saying. But surely we should take his claims at face value, and insist that circular argument does not become acceptable simply by a denial of its being vicious. Thagard's appeal to the criteria of coherence to justify the criteria of coherence is unconvincingly circular if anything is. Since circular arguments are unconvincing in general, we should be disinclined to accept a circular argument for Thagard's criteria. I doubt, then, that Thagard can escape the dilemma of naivete or circularity raised in Section 1.

6 DEDUCTIVISM AND EPISTEMOLOGY

The Limitations of Deductivism includes essays by Hempel, Giere, Salmon, Kyburg, and Suppe on the extent to which modes of reasoning and explaining in the natural sciences are deductive. No specific epistemological theme unifies this collection, but the contributors generally agree that deductive schemas for scientific confirmation and explanation need supplementation. I shall have to limit myself to the highlights of Hempel's lead essay.

In 'Provisos', Hempel suggests that a deductivist construal of scientific inference has three main steps. First, one interprets an observed state of affairs, deductively, according to the terms of some theory. Second, one makes a deductive inference, within the theory, from the initial theoretical statement to some theoretical prediction. Third, one deductively construes this inferred theoretical statement in observation terms to arrive at a confirmable prediction.

Hempel objects to deductivism on two main grounds. First, deductivism faces a problem of 'theoretical ascent': the initial step of interpreting an observed state of affairs according to a theory seems not to be deductive. Hempel illustrates:

> [T]he theory of magnetism surely contains no general principle to the effect that when iron filings cling to a metal bar, then the bar is a magnet. The theory does not preclude the possibility, for example, that the bar is made of lead and is covered with an adhesive to which the filings stick. . . . Thus, the theory does not warrant a deductive transition . . . (pp. 21–2).

Second, deductivism faces a problem of 'provisos': the inference, via rules of interpretation, from a prediction within a theory to a confirmable observation statement seems not to be deductive. Hempel illustrates:

> The theory [of magnetism] clearly allows for the possibility that two bar magnets, suspended close to each other at the same level by fine threads, will not arrange

> themselves in a straight line. For example, if a strong magnetic field of suitable direction should be present in addition, then the bars would orient themselves so as to be parallel to each other (p. 22).

Hempel notes that the inference in question requires the assumption that the suspended magnets are not subject to any disturbing influence. An assumption of this sort is a *proviso.*

Hempel explains the important consequences of the role of provisos for (i) the idea of falsifiability, (ii) elimination programs for theoretical terms, (iii) instrumentalist construals of scientific theories, and (iv) the notion of a theory's 'empirical content'. First, a scientific theory is not normally falsifiable by observation statements. Second, theoretical expressions are generally unavoidable in a scientific theory. Third, we should reject instrumentalist approaches according to which an input of observation statements generates a certain output of observation statements. Fourth, scientific theories do not have 'empirical content' to the extent typically assumed.

Hempel concludes with a methodological lesson: the appraisal of provisos in scientific theories supports Kuhn's view that scientific theory-choice is not subject to precise explicit criteria. Thus: 'In the context of theory choice, the relevant idea of the superiority of one theory to another has no precise explication, yet it is not arbitrary, for its use is strongly affected by considerations shared by scientific investigators' (p. 34). Unfortunately, Hempel does not provide a detailed account of these shared considerations. So it is unclear how close his view comes to Kuhn's community authoritarianism outlined in Section 2. But it is clear that Hempel has moved significantly in the direction of Kuhn.

I doubt that deductivism has much hope of survival after Hempel's arguments on the significance of provisos. But I am not convinced that Kuhn's views on theory-choice thereby receive support. For Hempel has not shown that what a scientific community means by 'justification', relative to its cognitive goals and candidate theories, must exclude precise criteria for a theory's being justified. Hempel's essay does support the view that observational considerations by themselves will often fail to settle questions about confirmation. But this view does not entail that observational considerations *plus* specific conceptual or theoretical considerations will also fail to settle, in a precise way, epistemological issues in the natural sciences. Thus, so far as Hempel's provisos go, it seems to be an open question whether Kuhn's views on theory-choice are actually correct.

7 CONCLUSION

This review has confirmed the importance of meta-epistemology in the philosophy of science. We have seen, at various places, that theorists give inadequate attention to meta-epistemological issues of the sort raised in Sections 1 and 2. Epistemology of the natural sciences suffers as a direct result.

The outstanding meta-epistemological issues can be treated adequately, I suggest, only by attention to the *semantics* of justification-talk. That is, we must decide up front what we *mean* by 'scientific justification' before engaging in epistemological debate with opposing theorists. For, I submit, the giving of a justification will be convincing only if its presumed conditions for justification are logically connected to what one means by 'justification'. An ultimate appeal to such meaning will avoid the sort of naivete and circularity identified in Section 1.

Specifically, the giving of justification, on my proposal, will take the following form:

1. By definition, justification (for either explanatory or evaluative standards for scientific justification) consists in conditions *C*.
2. Standard *S* satisfies *C*.
3. So standard *S* is justified.

This is an argument schema for one's *giving* justification, not for one's merely having justification. Merely having justification seems not to require giving an argument of any sort. Premise 1 has its basis in certain definitional rules for 'justification'. In giving justification, we use such rules as the basis for our appeal to definitional truth. I call the present approach to giving justification *semantic foundationalism*; its main thesis is that the giving, explaining, and evaluating of justification terminate with what one means by 'justification'. On this approach, one's definitions of 'justification' are not themselves candidates for epistemic justification by considerations independent of fixing meaning; they rather are preconditions for one's justifying anything.

Our ultimate appeal in giving justification of any sort must be to a notion of justification that links the justifying considerations invoked with the giving of *justification*. We should not assume, then, that justification for standards or rules can come simply from objective, conceiver-independent structures of the world. For appealing to anything *X* (*e.g.*, natural kinds, universals, reliable mechanisms, physical features) to provide justification presupposes a prior *notion* of justification relative to which *X* can and does provide justification. Omit the presupposed notion, and the claim to provide justification via *X* will be logically inconclusive. Whatever constraints we link to justification, *we* are making the linkage relative to our notion of justification. This is one of the main lessons of semantic foundationalism.

Semantic foundationalism allows for what I call *semantic relativism*: two theorists can have different definitional rules for the use of the term 'justification'. This implication should be no surprise; in fact, I regard it as a platitude in light of the history of epistemology. Semantic relativism does not, however, entail substantive normative relativism. Relative to some notions of justification, epistemic requirements have no cultural or personal variability; their requirements are universal. So, semantic relativism does not entail an 'anything goes' attitude toward epistemic assessment. In other words, the rejection of normative relativism leaves semantic relativism untouched. (For further elaboration on semantic foundationalism, see my [1991].)

An appeal to semantic meaning will make explicit the decisive role of a decision-making philosopher in an epistemology or a philosophy of science. An epistemology is, after all, the product of some individual, and so it is only natural that an epistemological story end with an acknowledgment of what its author means by 'justification'. This is not Carnap's apriorism, however, since we need to recognize that scientific justification is a function of actual scientific goals. If we disregard these goals in our notion of justification, we are no longer talking about *scientific* justification, at least in the sense relevant to actual scientists. As for the rationality of scientific goals themselves, I must leave this complex topic as unfinished business.

I conclude with a general consideration in favor of semantic foundationalism. Either our substantive explanatory and evaluative views in epistemology are logically linked to our semantic views or they are not. If they are, our semantic views can provide the epistemic anchor identified by semantic foundationalism. If they are not logically linked, our substantive epistemology will become either empirical or synthetic a priori. For reasons familiar from Ayer, Waismann and others, it is unclear how any synthetic truths can be justified a priori. Specifically, it is unclear how statements true or false in virtue of nondefinitional considerations can have warrant apart from experiential considerations of some sort. Similarly, it is doubtful that empirical considerations can solve the longstanding problems of epistemology. An appeal to empirical considerations *pre*supposes the settling of pressing issues about justification in epistemology. For one must have a notion of justification that enables the relevant empirical considerations to do the epistemological work one desires. Overall, then, semantic foundationalism is probably the best remedy for our meta-epistemological headaches.

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REVIEW ARTICLE

Francis Bacon and the Disputations of the Learned*

PETER URBACH [1987]: *The Philosophy of Francis Bacon*, La Salle, Illinois.

Condorcet wrote in 1793 that Bacon had discovered 'la véritable méthode pour étudier la Nature;[1] in 1810 Goethe doubted that one could even speak of a Baconian 'method';[2] in the early Victorian period William Whewell hailed the Lord Chancellor as 'the supreme Legislator of the Modern Republic of Science',[3] whereas, at the end of the century, Thomas Huxley stigmatized the claims of Baconian philosophy as 'pseudoscientific cant'.[4] These contrasting views find their no less sweeping counterparts in our century, though the student is more likely to encounter abuse than praise. So in 1968, L. Laudan remarked that 'there are probably more studies devoted to Bacon than to any other figure in the history of the philosophy of science'.[5] I think one can fairly guess that the situation has not substantially changed in the intervening two decades.[6] Viewed either as a fraud or as the fountain-head of modern scientific rationality, the Baconian movement continues to inspire a great deal of interpretative effort. True, some of the most pressing questions have been convincingly answered by T. S. Kuhn in his seminal article 'Experimental

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* Bacon's works are cited from *The Works of Francis Bacon*, R. L. Ellis, J. Spedding and D. D. Heath (*eds*), 14 volumes (London, 1857–74; Stuttgart—Bad Cannstatt, 1963).

[1] *Esquisse d'un tableau historique des progrès de l'ésprit humain* [1795], A. Pons (*ed.*) (Paris, 1988), p. 210. Characteristically for an 18th-century *philosophe*, Condorcet adds in one breath that 'Bacon, qui possédait le génie de la philosophie au point le plus élevé, n'y joignait point celui des sciences; et ces méthodes de découvrir la vérité, dont il ne donne point d'example, furent admirées des philosophes, mais ne changèrent point la marche des sciences' (*ibid.*).

[2] *Materialien zur Geschichte der Farbenlehre* [1810], *Vierte Abteilung*, in *Goethes Werke*, hrsg. Erich Trunz (Hamburger Ausgabe, Munich, 1981), Volume XIV, p. 90. Goethe claims that Bacon's influence has been '*mehr schädlich als nützlich*' (*ibid.*).

[3] *The Philosophy of the Inductive Sciences* [1840], G. Buchdahl and L. Laudan (*eds.*) (London 1967) from the 1847 edn., Part II: VI, p. 227.

[4] *Lectures and Essays* (London, 1908), p. 46.

[5] Theories of Scientific Method from Plato to Mach: A Bibliographical Review' [1968]: *History of Science* 7, 1–63.

[6] A commented review of recent Baconian literature is provided by Paolo Rossi in 'Ants, Spiders, Epistemologists', in *Francis Bacon: Terminologia e fortuna*, Marta Fattori (*ed.*) (Rome, 1984), pp. 245–60.

versus Mathematical Tradition in Western Science',[7] but there remains a host of queries that historians and philosophers keep considering for the purpose of criticism and re-assessment. As is normally the case with any such revisions, historiography recapitulates genealogy, and the endeavour to establish (or perhaps to blur) a pedigree looms very large amongst the philosophers' inspirations.[8] The search for ancestors appears as a well established *genre*, though Bacon did not commend the 'disputations of the learned' as the most likely place to discover truth (*In Praise of Knowledge*, 1592), and Joseph Agassi has exposed that search as one of the most pernicious dead-ends in the historian's path'.[9] The polemical stance of such books may invite further polemic but seldom satisfies the demands of rigorous scholarship.

Peter Urbach's book *The Philosophy of Francis Bacon* is an honest specimen of the last mentioned group. The author candidly announces that, in his view, Bacon well deserves the title of 'Father of Experimental Philosophy' (p. 185), and that 'his [Bacon's] method, which I call the "hypothetico-inductive method", is both good *and* original' (p. 15, his italics). Now it goes without saying that the starting-point of any such inquiry has to coincide with the author's own position in philosophical matters, for otherwise the nature of the praise bestowed on Bacon's supposed achievements would be wholly opaque. Thus, Peter Urbach seems to spouse a form of hypothetico-deductivism or reformed Popperianism which he has developed, among other places, in 'The Objective Promise of a Research Programme' (*Progress and Rationality in Science* [1978]: G. Radnitzky and G. Andersson (*eds*.), pp. 79–93) and, most conspicuously from a historiographic viewpoint, in 'Francis Bacon as a Precursor to Popper' (*The British Journal for the Philosophy of Science* 33 [1982], 113–32)—an article of which the present book seems to be a much enlarged version.

So far so good; but the professed persuasions of a philosopher-cum-historian (emblematically: a Cassirer, a Koyré, a Maier, a Kuhn . . .) should not be so powerful as to make the student forget that today's ideals and canons of rationality are not necessarily coextensive with those recognizable or, rather, *reconstructable* in the figures he or she is studying. The circle of interpretation is inescapable, since we always interpret past texts from the viewpoint of other (as a rule, recent) texts, but a modicum of hermeneutical awareness prohibits the automatic projection of today's categories into the object of study. In theory, everyone agrees with this trite warning, but one wonders if the very use of certain seemingly innocent expressions like 'philosophy of science' is not already vitiating the historiographic project. Undoubtedly, the profusion of such titles[10] is a welcome sign that English-language historiography is vigorously asserting itself in a domain—the philosophy of science as an academic discipline—which tended to despise past philosophies as necessarily irrelevant. But one should not forget that today's idiolect refers to a specific field of reflection which, say, Aristotle, Bacon, Descartes or Berkeley did not know of. That is why to establish a *topography of knowledge* is imperative in each case in order to see whether former epistemological insights or speculations about Nature, Man or God can be adapted to present-day idiom without unacceptable semantic distortion.[11]

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[7] *The Essential Tension* (Chicago, 1977), pp. 31–61, esp. pp. 41–52; originally published in *The Journal for Interdisciplinary History* [1976]: 7, 1–31.

[8] *Cf.* A. Pérez-Ramos, *Francis Bacon's Idea of Science and the Maker's Knowledge Tradition* (Oxford, 1988), pp. 7–31, on 'the Meanings of Baconianism'.

[9] 'Towards a Historiography of Science', *History and Theory* [1963] Beiheft 2, pp. 3–13. Agassi labels 'inductivism' that historiographic current and sees in it a form of up-to-date 'textbook worship' (p. 14).

Now, Urbach's claim that Bacon, 'in his own time and long after, was celebrated as a great and illustrious philosopher of science' (pp. 14 f.) is a blatant form of begging the question, stemming perhaps from a faulty historical record. The process of Bacon's cautious reception in France by Mersenne's circle and of his near deification as a token-figure by the English thanks to Thomas Sprat's *History of the Royal Society of London* [1667] is extremely complex, and it is rather from the category of the *ars inveniendi* that we may *partly* account for the vagaries of his reputation as a self-professed methodologist.[12] Further yet, nationalist rhetoric and the urgent need for an ideology of science as a *locus neutrus* of gentlemanly pursuit played a non-negligible part in the Baconian clothing of natural philosophy in Newton's age.[13] And even in Victorian England, Whewell's or Herschel's laudatory references to Bacon were *not* the result of straightforward reading, but largely responded to the demands of internal polemic.[14] If, on the other hand, we turn to Bacon's own texts, we shall certainly find a wealth of pronouncements on such topics as the faculties of the human mind, the causes of error and its typology, the relation between man and the world, the character and limits of human knowledge, the methods of natural inquiry, the nature of matter, the interpretation of ancient myths and so on. Now it is highly debatable that, however amenable to today's idiom, Bacon's reflections on such topics are remotely those of a 'philosopher of science' in our sense of the term. Many of Descartes' speculations and most of Berkeley's *may* qualify as such: in the former case because Descartes himself systematically elaborated a scientific corpus; in the latter because, from *An Essay towards a New Theory of Vision* [1709] onwards, Berkeley was bent on analyzing and criticizing some of the key-concepts of contemporaneous science (viz. Newtonian optics or mechanics), with its putative philosophical presuppositions and implications. Bacon, on the other hand, was not philosophizing 'on science' in that sense. Rather, for him 'science' (*i.e. scientia, philosophia*. . .) lay well ahead and was an entity in the making.[15]

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[10] As an example *cf.* the excellent works by D. M. Clarke, *Descartes' Philosophy of Science* (Manchester, 1982) and Richard R. Brook, *Berkeley's Philosophy of Science* (The Hague, 1973). The title 'Francis Bacon's Philosophy of Science' has been already used in two articles: one by C. J. Ducasse in *Theories of Scientific Method from the Renaissance Through the Nineteenth Century*, E. H. Madden (*ed.*) (Washington, 1966), pp. 56–74; and another by M. B. Hesse in *A Critical History of Western Philosophy*, D. J. O'Connor (*ed.*) (New York, 1964) pp. 141–52, also in *Essential Articles for the Study of Francis Bacon*, ed. B. Vickers (London, 1972), pp. 114–39.

[11] The origins of the stance 'philosophy of science' have been studied by G. Buchdahl in *Metaphysics and the Philosophy of Science. The Classical Origins. Descartes to Kant* (Oxford, 1969); N. Jardine has narrowed the scope of that inquiry in his admirable study on Kepler's epistemology *The Birth of History and Philosophy of Science. Kepler's 'A Defence of Tycho Against Ursus' with Essays on its Provenance and Significance* (Cambridge, 1984), esp. pp. 258–86. As to the evolution of the notion of 'theory of knowledge' into present-day 'philosophy of science', *cf.* J. Habermas, *Erkenntnis und Interesse* (Frankfurt-on-Main, 1968), translated as *Knowledge and Human Interests*, by J. J. Shapiro (London, ^{2}1978), pp. 67 ff.

[12] *Cf.* T. Kisiel, 'Ars inveniendi: A Classical Source for Contemporary Philosophy of Science', *Revue Internationale de Philosophie* [1980]: 34, 130–54.

[13] *Cf.* M. Blay, 'Remarques sur l'influence de la pensée baconienne à la Royal Society: Pratique et discours scientifique dans l'étude des phénomènes de la couleur', *Études Philosophiques* [1985], 359–73, and *La Conceptualisation newtonienne des phénomèmes de la couleur* (Paris, 1983); also M. Malherbe, 'L'induction baconienne: de l'échec métaphysique à l'échec logique', in *Francis Bacon: Terminologia e fortuna, op. cit.*, pp. 179–200.

Bearing these reservations in mind, the reader can approach Urbach's book as a systematic attempt to conduct a debate between certain contemporary positions and an almost contemporary Francis Bacon—'if Bacon is to be *rescued* as a significant contribution to the philosophy of science' (p. 39, my italics). This historiographic *parti pris* allows the author to re-examine what he terms the 'standard interpretation' of Bacon's method, or 'infallible mechanical thesis' (p. 20) in order to refute it and, since that thesis is reputedly wrong, he further argues that 'Bacon's contribution to the understanding of *scientific method* . . . is *interesting*, largely *original* and, as far as it goes, substantially *correct*' (p. 24, my italics). Of course, there are no a priori grounds to dismiss this approach or to challenge that verdict, but if we have already detected a certain hermeneutical naïvety in Urbach's use of the label 'philosophy of science', we should perhaps point out that the notion of 'scientific method' does not immediately show its intellectual credentials, and hence the existence of any such entity should be briefly discussed.[16] Moreover, since 'scientific method' is more or less tacitly identified with a Popperian approach in natural inquiries, and the author presents a version of it which stresses its differences from conventionalism (p. 32, pp. 56 ff.), then a further difficulty arises, for the reader should not take Urbach's reading of Popper on trust. Granted that Urbach is writing on Bacon and not on Popper, but if the latter is the yardstick by which to gauge the former, then no over-simplification should be allowed, since both the author's version of conventionalism and of Popperianism have been radically questioned—historically and philosophically.[17] For example, it is simply not true that ancient astronomy *unqualifyingly* obeyed an 'instrumentalist' or conventionalist' inspiration, as Duhem argued long ago[18] and Urbach repeats (pp. 30 ff., pp. 125 ff.); nor for that matter, do the notions of 'epicycle' (*qua* theoretical entity) and 'atom' (*qua* unobservable entity) stand on a par as regards their conceptual import (p. 31) or as Bacon treated them.[19] And, as far as Popper is concerned, his own reflections were originally prompted as a reaction against 'verificationism' and 'positivism' (*Unended Quest*, London 1974, p. 62) rather than as a critique of conventionalism, as Urbach claims (p. 56).

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[14] *Cf.* R. Yeo, 'An Idol of the Market-Place: Baconianism in 19th-century Britain', *History of Science* [1985]: 23, 251–98; B. Lanaro, 'Il genio e le regole. Osservazioni su Whewell e l'immagine di Bacone nel primo Ottocento', *Rivista di storia de la filosofia* [1989]: 44, 37–67; and A. Pérez-Ramos, *Francis Bacon's Idea of Science, op. cit.*, pp. 20–27.

[15] The origin of the term 'scientist' illustrates the gradual self-understanding of 19th-century 'science' as an autonomous stance. *Cf.* S. Ross, '*Scientist*: The Story of a Word', *Annals of Science* [1964]: 18, 65–85; and M. N. McMorris, 'Science as *Scientia*', *Physis* [1981]: 23, 171–96.

[16] *Cf.* as a first approximation Ernan McMullin, 'Is There a Well Defined Scientific Method?', *Journal of the Minnesota Academy of Science* [1967]: 34, 22–27; and, despite its much provocative character, P. Feyerabend, *Against Method* (London, 1975). For a practical example of the extreme elusiveness of that category ('scientific method') and its historical negotiations, *cf.* S. Shapin and S. Schaffer, *Leviathan and the Air Pump. Hobbes, Boyle and the Experimental Life* (Princeton, 1985), esp. 332–44.

The 'standard interpretation of Bacon', the author argues (pp. 19 f.), is characterized by a definite view of the *aim* and *method* of Baconian science. Its aim is to apprehend the Forms (*Nov. Org.* II, 3) and its method is observation (I, 387), experiment (I, 386, 624–33) and, above all, induction (I, 236, 256). The first part of this interpretation[20] has long been denounced as a historical paradox, since no concept in modern science remotely resembles the Baconian Form;[21] as to the second, the claim that induction could proceed on a limited number of observations and experiments and thus arrive at infallible results has long been rejected as an illusion.[22] Why is Bacon *so* important then, if even his near contemporaries like Descartes, Mersenne or Gassendi, no less than Sprat or Boyle, remained unconvinced by his supposedly most characteristic contribution? Urbach tries to answer this question by a non-historical and normative route: 'Bacon welcomed hypotheses (in the sense of theories going beyond what is immediately given in perception) from the very beginning, and . . . they [*i.e.* the hypotheses] were always the intended product of the interpretative method [*i.e.* Bacon's *Interpretatio Naturae*]' (p. 34). Induction, as I understand Urbach, then functions as a kind of observational strategy whereby hypotheses should prove their mettle so that, in the end, a sort of 'moral certainty' (p. 41) can be attained as to which of them is the most likely to be promoted to the rank of scientific truth.[23] This, Urbach contends, shows 'striking similarities' (*ibid.*) with Popper's conception, for both Bacon and Popper supposedly regard scientific theories as hypothetical and require them to be '*independently testable*' (p. 50, Urbach's italics). Moreover, Bacon, unlike Popper, spoused a kind of 'gradualistic approach' (pp. 54 ff.) as pertains to the

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[17] *Cf.* Jerzy Giedymin, 'Instrumentalism and its Critique: A Reappraisal', *Boston Studies in the Philosophy of Science*, R. S. Cohen *et al.* (*eds.*) [1976]: 39, 179–207, and 'Radical Conventionalism, its Background and Evaluation: Poincaré, Le Roy and Ajdukiewicz', *Science and Convention* (Oxford, 1982), pp. 109–48; Elisabeth Ströker, 'Does Popper's Conventionalism Contradict His Critical Rationalism? Objections Against Popper in German Philosophy and Some Metacritical Remarks', in *Methodology, Metaphysics and the History of Science*, R. S. Cohen and M. Wartofsky (*eds.*) (Dordrecht and London 1984), pp. 263–82; E. Zahar, 'Positivismus und Konventionalismus', *Zeitschrift für allgemeine Wissenschaftstheorie* [1980]: 11, 292–301; and the book by Giovanni Rocci, *Scienza e convenzionalismo* (Rome, 1978).

[18] Duhem's position has been challenged from a philological viewpoint by G. E. R. Lloyd, 'Saving the Appearances', *The Classical Quarterly* [1978]: 28, 202–22; Nicholas Jardine has provided an overview of the issue in 'The Forging of Modern Realism: Clavius and Kepler Against the Sceptics', *Studies in History and Philosophy of Science* [1979]: 10, 141–73, where he shows the limited extent to which the Duhemian account is to be accepted.

[19] Epicycles are called 'fictions' in III, 741 and V, 517. On the other hand, atoms are the 'smallest portion of the division of bodies' (III, 171), really existing 'far beyond what is visible' (V, 419 f.). In that sense, the idea of the atom is 'necessary' (V. 492).

[20] One can question the view, however, that such is the 'standard interpretation' nowadays. Paolo Rossi's book (whose Italian original *Francesco Bacone: dalla magia alla scienza* goes back to 1957) was translated into English in 1968. The works of N. W. Gilbert (*Renaissance Concepts of Method*, New York, 1960), Cesare Vasoli (*L'enciclopedismo del Seicento*, Naples, 1978) and Angelo Crescini (*Il problema metodologico alle origini della scienza moderna*, Rome, 1972) have also contributed to forge a new *Baconbild* by connecting Bacon's works with Renaissance traditions in rhetoric and techniques of communication. L. Jardine (*Francis Bacon. Discovery and the Art of Discourse*, Cambridge, 1974) has explored this track. In the field of philosophy of science proper, the original studies of L. J. Cohen have dispelled many prejudices about the

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'standard interpretation', especially as regards the notion of a Baconian as against Pascalian probability. *Cf.* L. J. Cohen, *The Probable and the Provable* (Oxford, 1977), and 'Some Historical Remarks on the Baconian Conception of Probability', *Journal of the History of Ideas* [1980]: 41, 219–31.

[21] As Descartes' editor Pierre Tannery wrote in 1891: 'Le point essentiel dans les ouvrages du Chancelier, c'est sa théorie des formes, nullement sa méthode, qui d'ailleurs n'est vraiment compréhensible que si le but est l'invention des formes, telles que l'entendait Bacon; or . . . ces formes ne répondent à rien dans la science moderne', *Revue Philosophique* [1891], ii, p. 106; *cf.* also Charles Adam, *François Bacon* (Paris, 1890), pp. 76–80, 92–9, 289–304. Bacon's great editor R. L. Ellis contended that the theory of Forms was extraneous to the Lord Chancellor's philosophy (*General Preface to the Philosophical Works*, I, 28 ff.).

[22] Cf. Galileo Galilei: 'If induction had to take up all the particulars, then it would be impossible or useless; it would be impossible when the particulars were innumerable; and when the particulars were numerable, to consider them all would render any conclusion by induction useless or, to put it better, null (*inutile o, per meglio dire, nullo, il concludere per induzione*)', *Risposta a Vincenzo di Grazia*, in *Opere*, A. Favaro (*ed.*) (Florence, 1890–1909), IV, p. 701. This position has to be seen against the background of the Scholastic distinction between *inductio perfecta* and *inductio imperfecta*: *cf.* A. Pérez-Ramos, *op. cit.*, pp. 216–24. *Cf.* also J. R. Milton, 'Induction before Hume', *The British Journal for the Philosophy of Science* [1987]: 38, 49–74. On Mill's and Whewell's general conception of induction and their criticisms of Baconian induction, *cf.* Giorgio Lanaro, *La teoria dell'induzione in William Whewell* (Milan, 1987), esp. 81–110, 182–88.

[23] Urbach uses the phrase 'moral certainty' as though it were immediately given in the language. The reader should be reminded that 'moral certainty' is a *precise* epistemological category forged in the 17th century in the context of a long philosophical debate *partly* initiated by Bacon and Descartes. *Cf.* H. G. van Leeuwen, *The Problem of Certainty in English Thought (1630–1690)* (The Hague, 1963), pp. XII, 74 ff., 144; and B. J. Shapiro, *Probability and Certainty in 17th-Century England. A Study of the Relationship between Natural Science, Religion, History, Law and Literature* (Princeton, 1983), pp. 18–38, 267–72.

acceptance of candidate theories, so that mere testability is not a sufficient condition of scientificity:

> Popper believed that the scientist should be as daring or 'bold' as possible in advancing theories, while Bacon took the view that caution is preferable, and that one should approach theories of increasing generality by gradual stages (p. 56).

In view of which the author is bound to conclude that 'Bacon's circumspect, step-by-step approach is the more reasonable' (*ibid.*). I suspect the reader expected that much, and this quotation is apt to encapsulate the merits and limitations of Urbach's book: Bacon and Popper are partners in an atemporal philosophical dispute where a clear but, alas, trivial verdict ('more reasonable') is forthcoming. As I intimated above, this approach has been vitiated by the author's hermeneutical *parti pris* and, therefore, the conclusions of the whole book stand or fall with it. True, the critic can always isolate certain patterns of reasoning such as those Urbach resorts to, and show that they do belong to Bacon's *idea of science* as well as to Popper's, though their contexts are radically different. Now, if such contexts are not reconstructed, then no history of philosophy worthy of that name has been written and, in that respect, Urbach's book scarcely qualifies as a book of philosophical scholarship. Let us see some of the reasons why.

First, we have the question of falsificationism, which Urbach takes to be central to his discussion, even as he emphasized it in his article on 'Bacon as a Precursor to Popper'. Indeed, the asymmetry of verification and falsification was known to Bacon and, as Adolf Grünbaum has convincingly shown,[24] to most so-called inductivist methodologies. Here is one of Bacon's various pronouncements on the subject in *Nov. Org.* I, 46:

> And therefore it was a good answer that was made by one who, when they showed him hanging in a temple a picture of those who had paid their vows as having escaped shipwreck and would have him say whether he did not acknowledge the power of the gods—'Aye', asked he again, 'but where are they painted that were drowned after their vows?' And such is the way of all superstition, whether in astrology, dreams, omens, divine judgement (*nemesibus*), or the like; wherein men . . . mark the events where they are fulfilled (*advertunt eventus ubi impletur*), but where they fail . . . neglect and pass them by (*negligunt et praeterunt*) (VI, 56; I, 166).[25]

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[24] *Cf.* A. Grünbaum, 'Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper *versus* Inductivism', *Boston Studies in the Philosophy of Science* [1976]: 39, 213–52; J. W. N. Watkins, 'The Popperian Approach to Scientific Knowledge', *Progress and Rationality in Science*, G. Radnitzky and G. Andersson (*eds.*) (Dordrecht and Boston, 1978), pp. 23–43.

[25] For an analysis of this type of argumentation, *cf.* G L. Linguiti, 'Induzione e deduzione: riesame del Bacone popperiano', *Rivista di filosofia* [1978]: 69, 499–515, and A. Pérez-Ramos, *op. cit.* 239–69.

Likewise in his *Logic* (III, 10, § 1) J. S. Mill warned the reader against 'the tendency of unscientific observers', who

> rely too much on number, without analyzing the instances; without looking closely enough into their nature to ascertain what circumstances are or are not eliminated by means of them. Most people hold their conclusions to a degree of assurance proportional to the mere *mass* of the experience on which they appear to rest; not considering that by the addition of instance to instance, all of the same kind . . . nothing whatever is added to the evidence of the conclusion. A single instance eliminating some antecedent which existed in all the others is of more value than the greatest multitude of instances which are reckoned by their numbers alone.[26]

So that even a 19th-century textbook like Thomas Fowler's *Elements of Inductive Logic* (Oxford, 1882) lists this topos amongst the most frequent 'fallacies incident to induction', *i.e.* the unqualified stress on verification (pp. 239 f.). Hence this alleged Baconian discovery (*Major est vis instantiae negativae*: *Nov. Org.* I, 46) is by no means an isolated case in the history of philosophy or in the practice of scientific disciplines by 17th-century philosophers.[27] In general, the use of *modus tollens* types of argument is well attested in former ages, though I grant that a whole historiographic current has blurred its trace when stressing the verificationist components of Bacon's methodological thought.

Yet, there are at least two crucial aspects of Bacon's *ars inveniendi* that have escaped Urbach's notice. The first is the widespread use of analogical arguments as the unwarranted leap of the intellect whenever the Baconian inquirer attempts to delineate a Form. This use of analogy is by no means restricted to the so-called *instantiae analogiae*, which Bacon places amongst the Prerogative Instances of *Nov. Org.* II, 27 and Urbach discusses on p. 167. Rather, the resort of analogical patterns of reasoning is embedded, Bacon contends, in the workings of any inquiring mind as an indispensable tool of research. Thus, the inchoate examples of the Forms of heat or of whiteness (IV, 127–55, 361; I, 236–68, 270, 566; III, 236–8) which Bacon offers as examples of the deployment of induction, are themselves the result of an unaccounted-for passage from the realm of what is accessible to sense (and hence can be organized in the *tabulae* of *Nov Org.* II, 11, 12 and 13) to what is inaccessible, viz. the corpuscles and their motions. Hence it has to be *imagined* or made visible as a theoretical model on the analogy with what is accessible.[28] Secondly and far more importantly, Urbach has not detected the hermeneutical import of the Baconian concept *opus* (or 'works' in Bacon's own English), and so he interprets Bacon's demand for scientific productivity in the old utilitarian or pragmatic sense: either as a plea for confirmation in applied science or, in the form of *experimenta lucifera* (*Nov. Org.* I, 70, 97), as offering pieces of evidence shedding more light on new investigations (*cf.* Urbach's use of 'sterile' and 'unproductive' on p. 56). Now, since I myself have studied this question in my book on Bacon, I can only refer the discerning reader to my work for an assessment of this topic.[29] A brief gloss, however, may be useful here: I take the notion of *works*, of *opera* to be pivotal in Bacon's thought inasmuch as it connects the twin concepts of *Naturerkenntnis* and *Naturherrshaft* in the dawn of the Modern Age. That is to say, it unfolds the notion of *knowledge* as both derived from and leading to *power* over Nature. Bacon's 'Works' are not artefacts or utensils, but the results of active *directions* (a Ramist technical term that Urbach has overlooked on pp. 26 f.) or recipes for (re-)producing that very natural 'effect' which the inquirer has been studying (*Nov. Org.* 1, 5, 81). That is, the ideal of science inspiring Bacon's methodological insights stands for a kind of knowledge which enables the knower to make or (re-)produce that which he knows in the form of theoretically-informed recipes for action. In Bacon's own opaque terms from the early *Valerius Terminus* (*c.* 1603).

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[26] J. S. Mill, *A System of Logic, Ratiotionative and Inductive* (London, ^{8}1884), p. 287 (author's italics).

[27] *Cf.* Blaise Pascal: 'Dans toutes les matières dont la preuve consiste en expériences et non en démonstrations, on ne peut faire aucune assertion universelle que par la générale énumeration de toutes les parties ou de tous les cas différents . . . S'il restait un seul cas à examiner, ce seul suffirait pour empêcher la définition générale, et si un seul était contraire, ce seul . . .' (*De l'Esprit géométrique . . . et autres textes*, A. Clair (*ed.*) (Paris, 1985, p. 63); and A. Arnauld et P. Nicole, *La Logique ou l'Art de penser* (1662; L. Marin (*ed.*) Paris 1970), Chapter XIX, pp. 321–2. The importance of the anomalous case (*Urphänomen*) as the springboard for the exercise of reason in scientific inquiry was clearly emphasized by Goethe in *Materialien zur Geschichte der Farbenlehre*, *op. cit.* p. 91, where he writes against Bacon: 'Wer nicht gewahr werden kann, daß ein Fall oft tausende wert ist, und sie alle in sich schließt, wer nicht das zu fassen und zu ehren imstande ist, was wir Urphänomene genannt haben, der wird weder sich noch andern jemals etwas zur Freude und zum Nutzen fördern können', *op. cit.*, pp. 91 f. Goethe's misunderstanding is blatant: nowhere does Bacon assert that all instances are of equal value and indeed the *instantiae solitariae* in *Nov. Org.* II, 22 are amongst those in which, as Goethe puts it, 'one case is worth a thousand'. I suspect that Goethe's animus against Bacon was a byproduct of his polemic against Newton, since Newton's *Opticks* passed as a work of Baconian inspiration.

> To make a stone bright or make a stone smooth it is a good direction to say, make it even, but to make a stone even, it is not a good direction to say, make it bright or make it smooth; for the rule is that the disposition of anything referring to the state of it in itself or the parts, is more original than that which is relative or transitive towards another thing. So evenness is the disposition of the stone in itself, but smooth is to the hand and bright to the eye, and yet nevertheless they all cluster and concur (III, 240).

In other words: to know *what*, for Bacon, is tantamount to knowing-*how*, or rather, it is the process of knowing-*how* that sustains or creates the theoretical space of man's natural science. Now this notion (*maker's knowledge, ergetic ideal*) enables us to understand the exact character of the Baconian critique of previous philosophies, and reveals the main import of his key-concept, viz. Form. In a nutshell: Bacon's Forms are praxiological statements purporting to encapsulate (provisional) what-statements and (successful) how-statements, *i.e.* descriptions of the 'effect' under investigation and instructions for its active (re-)production by man.[30] To *know* means to be able to *make*. The roots of this fruitful insight go very deep in the history of Western thought, though, as a model of knowledge, it has been made explicit only rarely—paradigmatically, in the Vichian formula *verum ipsum factum*. Yet, the notion was not unknown to Bacon's near contemporaries, partly as a response to Sceptical attacks,[31] and the vast success of the Baconian project of inquiry echoed the translation of this gnoseological inspiration (maker's knowledge) into the concrete tenets of the new mechanistic science. As we know, foremost amongst its methodological *mots d'ordre* was the active engagement of the researcher into Nature's processes. Needless to add, this radical reorientation of man's cognitive capacities conveyed the deployment of a novel arsenal of categories bent on interpreting Nature according to its new dictates: Nature is gradually stripped off her qualitative cloak and viewed as a piece of *manipulable* machinery.[32] Now, the emergence of this type of rationality was the upshot of many conflicting efforts, but Bacon's *Novum Organum*, over and above its strictly methodological contents, stands out as one of the most decisive contributions to the crystallization of that novel ideal.

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[28] Both M. B. Hesse (*loc. cit.*) and M. Horton ('In Defence of Francis Bacon: A Criticism of the Critics of the Inductive Method', *Studies in History and Philosophy of Science* (1973): 4, 241–78) have stressed this crucial aspect of Bacon's *ars inveniendi.*

[29] P. Rossi had already intimated this approach in *Philosophy, Technology and the Arts in the Modern Era [I filosofi e le macchine]* (1962; transl. S. Attanasio: New York, 1970), Appendix II on 'Truth and Utility in Bacon', pp. 148–73. *Cf.* A. Pérez-Ramos, *op. cit.*, esp. pp. 135–98, and *Conclusion*.

This all-important Baconian insight finds its earliest expression in *Valerius Terminus*, where the purely utilitarian bent of the new philosophy is repudiated:

> The discovery of *new works and active directions* not known before is the only trial to be accepted . . . the nature of this trial is not whether the knowledge be

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[30] The phrase 'ergetic knowledge' has been coined by A. Funkenstein in his admirable *Theology and the Scientific Imagination from the Middle Ages to the Seventeenth Century* (Princeton, 1986), pp. 290–345. *Cf.* my essay-review 'And Justify the Ways of God to Men', *Studies in History and Philosophy of Science* [1990]; 21, 323–339.

[31] Juan Luis Vives' terse insight 'Tantum scis quantum operaris' in *Satellitilium Animi* (1524 *Opera Omnia* (Valencia, 1782–90), G. Mayans (ed.), 8 vols, IV, p. 63; *cf.* C. G. Noreña, *Juan Luis Vives*, The Hague, 1970, Chapter IX.) finds an enormous echo in late 16th- and 17th-century philosophers like Sanchez, Mersenne, Gassendi, Descartes, Boyle, Hobbes, Locke and others. *Cf.* A. Pérez-Ramos, *Francis Bacon's Idea of Science, op. cit.*, pp. 167–96.

[32] Cf. A. Funkenstein, *op. cit.*, pp. 290–345; R. Lenoble, *Histoire de l'Idée de Nature* (Paris, 1969), pp. 309–37; and J. Mittelstraß, 'Das Wirken der Natur. Materialien zur Geschichte des Naturbegriffs', *Naturverständnis und Naturbeschreibung*, hrsg. F. Rapp (Munich, 1981), pp. 36–9, and 'Nature and Science in the Renaissance', *Metaphysics and Philosophy of Science in the 17th and 18th Century. Essays in Honour of Gerd Buchdahl*, R. S. Woolhouse (*ed.*) (Dordrecht and London, 1988) pp. 17–44; G. Freudenthal, *Atom und Individuum im Zeitalter Newtons. Zur Genese der mechanischen Natur- und Sozialphilosophie* (Frankfurt-on-Main, 1982).

> profitable or no, but even upon the point of whether the knowledge be true or no (III, 242, my emphasis).

No other is the sense of the Baconian stress (which Urbach fails to notice) on the propedeutic value of the mechanical arts, as already manifesting this ideal of knowledge. Arts and techniques, Bacon argues, constitute not only the visible embodiment of progress and perfectibility (I, 124–33; IV, 13–21; III, 226, 289–90; IV, 296, 298) but, pre-eminently, they exhibit the novel criterion of truth implicitly built into them. The knower is he who successfully engages himself in Nature's processes in order to (re-)produce them, rather than he who allegedly comprehends such processes or 'effects' in a conceptual grasp—as in the old Aristotelian *scientia*. And, in order to bolster up this new conception of science, Bacon declares the experiments of the mechanical arts to be 'by far the most important in philosophy' (V, 506; I, 730). That is why A. Funkenstein lucidly contends that 'the study of Nature in the XVIIth century was neither predominantly idealistic nor empirical. It was, first and foremost, *constructive*, pragmatic in the radical sense. It would lead to the conviction that only the doable—at least in principle—is also understandable'.[33]

As is normally the case with great philosophic achievements, Baconian doctrines not only unveiled patterns of thought which are deeply ingrained in man's cognitive praxis, but also forged an idiolect which was later put to other uses, such as the modern rhetoric of science and the ideology of technocratic success over Nature. By so doing, any philosophic discourse tightens up the field of reflection and is bound to reveal contradictions and blind alleys which its author could not have foreseen. But, of course, any study which posits Bacon's 'philosophy of science' as its starting-point is oblivious that the stance 'philosophy of science' has emerged well after this new image of Nature and the type of rationality accompanying it had been accepted. Thence its main concept (viz. 'science') takes for granted that past and present reflections of knowledge were largely coextensive, as past and present *Naturbegriffe*. This is a gross mistake.

I would not like to end up with this disparaging note. Urbach's book is an honest attempt at exploring a track which, however unsatisfactory, represents a healthy reaction against the vastly more unsatisfactory way, fashionable amongst too many philosophers of science, of treating past philosophies as irrelevant or naïve. The author has tried to overcome this prejudice by emphasizing the merits of one particular philosophy. He does so, however, at the price of oversimplification and anachronism. His urge to give such marks as 'interesting', 'original' or 'reasonable' is the stylistic symptom of an intellectual commitment which ignores the web of nuances whose exploration any genuine work of interpretation demands. Bacon can be read from Popper, true; but first he should be read from his contemporaneous context if we are really attempting to reconstruct past canons of thought, *i.e.* past meanings. Yet, *The Philosophy of Francis Bacon* is that book of *haute vulgarisation* that some contemporary philosophers of science can welcome as a healthy antidote against endogamic temptations. The book is clear, direct and unpretentious, as any first step in the route of conceptual analysis should be. To ask for more depth or scholarship would be perhaps to place it in the wrong context. Yet, there are many roads leading to the stronghold of rigorous study. Urbach's attempt is a worth-exploring path.

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[33] *Theology and the Scientific Imagination*, *op. cit*, p. 178 (author's italics).

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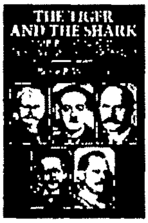

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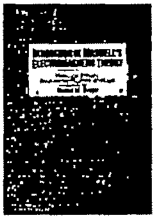

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